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VOL. 13, NO. 1 March, 1938

THE QUARTERLY REVIEW of BIOLOGY

WHAT ARE THE GENES?

I. THE GENETIC AND EVOLUTIONARY PICTURE

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INTRODUCTION

Genes as particulate bodies

MODERN studies in more than one branch of biology have brought into the foreground an intricate but not incomprehensible organized process underlying the phenomena of heredity.

There are five fairly obvious modes of scientific study of these phenomena: Experimental genetics, cytology of germ cells, chemical investigation of the constitution of nuclei, studies in evolution, and investigation of the developmental processes through which the genetic potentialities come into active expression. Each of these approaches has been used by scientists—the first two most intensively—and many valuable results achieved, especially where more than one of the methods has been brought to bear on a single aspect of the problem. The present paper and its sequel endeavor to place the problem of genes briefly under combined illumination from the diverse slants of cytology, genetics, biochemistry, and portions of the evolutionary field of study.

In the interpretation of the empirical data of heredity a leading rôle has been played by the concept of genes, or minute organized units of material located in their respective chromosomes, which exert a regulating influence on all the formative processes by which the new individual comes to express the characteristics of its species and variety. In its present form this concept comes jointly from experimental studies in breeding and from a close microscopical examination of the chromosomes and the transformations that they undergo (Morgan, 1928, etc.). In this way the particularistic nature of the hereditary process has been thoroughly verified, and the broader features have been determined in the mechanics of the distribution of the hereditary particles during the formation of the sex cells and their union into zygotes. Each unit of heredity, or gene, has its own distinct and peculiar influence upon the heritable characteristics of the whole organism. They are contributed to the zygote in essentially equal numbers by each gamete or sex cell, so that (except for the special case of X-

and Y-chromosomes, that are involved in the determination of sex) the hereditary influences of the male and the female parent are in the long run essentially comparable and evenly balanced (Jollos, 1935). This gives us confidence that genes belong in the chromosomes, which alone are equally represented in the germ cells of both sexes, and that they do not reside in the cytoplasm or in the yolk substance, since these latter are contributed by the female without seriously altering the balance of hereditary transmission. The motile flagellum or tail of the spermatozoon is also denied any serious rôle in the particulate determination of heredity, since it has no counterpart in the female, and the process by which it originates is unsuitable for producing the specialized assortment of genes which is revealed by the Mendelian laws.

Present-day geneticists are reasonably united in the conviction that the particularistic gene mechanism is concerned with virtually 100 per cent of what is heritable. This rather sweeping conclusion is the gradual outcome of a long search for evidence. The most effective argument from experiment is based on a comparison of inbreeding and outbreeding (Fisher, 1930). If heredity depended to any marked extent upon a matrix capable of being subdivided and reblended (as is implied in the ancient concept of ancestral "blood") then cross-breeding would have a powerful averaging effect, with an end product almost as uniform as that produced by inbreeding. But experiment reveals a degree of contrast between in- and outbreeding which points to a wholly or almost wholly particularistic background for heredity. (Green, 1933; Castle, *et al.*, 1936.) Hence the modern chromosomal-Mendelian concept is usually believed to apply to essentially the entire field of heredity in higher organisms.

The main outlines of the chromosomal theory and the arguments for belief in such "genes" as it postulates, have been covered with such care by so many authors, that it will be well for us to omit the greater part of their discussion and proceed after merely rehearsing certain conclusions which we take for granted as already scientifically established.

The genes are material bodies occurring in a linear sequence in their respective chromosomes.

The unfertilized sex cell carries a single series of genes which cover the whole range of heritable traits. The chromosomes carrying this series constitute what is called the "haploid" count.

Through the mechanics of chromosome distribution the different portions of this series are derived from various ancestral sources by a special kind of lottery.

The sexual union of the gametes endows the fertilized ovum and each of the resultant body cells with two such series. These cells then possess two *simple* or haploid counts making one "diploid" or *double* count of chromosomes amounting in all to a double set of the gene mechanism.

Within each of these two series, reduplications seem to be the exception rather than the rule for animals, although in higher plants there is often copious reduplication.

The serial order of genes is relatively fixed in any one species, and experimentally determinable. Alterations of this order, when they occur, are also experimentally verifiable.

The continuity of genes through the cell generations must be postulated to account for the retention of both normal and atypical serial arrangements.

Every part of this gene assemblage is indispensible, in the sense that if a fertilized ovum carries a simultaneous shortage of appreciable blocks of corresponding genes in its two series, such a double defect has always proved to be fatal.

The distribution of chromosomes is such that typically every chromosome furnished to the organism is distributed to all cells of the body, so that the genes it contains are given ubiquitous opportunities for exerting their influence.

Hereditary differences may doubtless sometimes consist in the presence or absence of single genes or pairs of genes, but they are more often referable to alterations of the constitution of some gene, such

that it becomes endowed with a different degree (or type?) of potency.

A change in the number of genes present of a single kind is at times indubitably the source of outstanding qualitative bodily differences. By far the most important example of this is the differentiation of sexes. In numerous groups of animals there is a block of genes—the "X" component, frequently present as a specialized chromosome—which determines development as a female if it is present in duplex, while if it occurs but once it is unable to inhibit the male developmental trend that is normally present in all cells. A few groups of animals have the reverse relation of chromosomes in the two sexes.

A differing space-arrangement of genes does not usually exert more than a trifling effect on somatic characters, although it may heavily disorganize the mechanics of sexual reproduction.

GENES IN MUTATION

From the standpoints of phylogeny, evolution and paleobiochemistry, one of the most important qualities of the gene mechanism is its liability to mutation, although from the detailed standpoint of the genetic relation between parent and offspring every occurrence of a mutation must be understood as representing some sort of mishap. The frequency of such genetic accidents may be increased experimentally by applying appropriate irradiation to the gonads. Within our theoretical pattern the mishaps may be of three possible types, either (1) a faulty distribution of the chromosomal material, so that certain blocks of genes are either omitted or reduplicated in the resultant germ cells, or (2) a rearrangement of the series, bringing certain genes into new juxtapositions which may supposedly condition some degree of change in the net efficiency of their mutual functions, or (3) some sort of qualitative or quantitative alteration of a single gene, without radical alteration of the general mechanism of the germ cell.

The fact has already been mentioned that changes in the *number* of genes may alter the character of the organism, and the mechanism of sex-determination has been cited in illustration. Many families of plants give fine illustrations of "mutations" that consist in the possession of redundant chromosomes, thus demonstrating that quantity differences in the equipment of genes will alter the visible traits of the species.

Accidental redundancies of this sort can provide a source for progressive increase in the genetic machinery beyond what was ancestrally present in the stock, but it obviously produces no gene of any new kind.

Mutations of the second type, by rearrangement without loss or addition, is of relatively minor importance, although throwing a light upon the manner of action of genes (Dubinin and Sidoroff, 1934).

Mutations of the third type, involving changes limited to single genes, have much the greatest interest for our problems. The greater part of the vast array of known Mendelian factors in animals is ascribed to this class. There is an almost universal tendency for new mutations of the sort found in the laboratory to be recessive as compared to the norm of the species. Since the effect of a gene and of its recessive allelomorph is usually after the manner of "all or nothing," a single dominant gene being approximately as effective as a pair, the relationship has commonly been interpreted in the past in terms of "presence" and "absence," a recessive being looked upon as the mere omission of a gene which when present is equally potent in single or double dose. There then follows Bateson's (1914) paradoxical corollary that observed mutations consist exclusively (or almost exclusively) in a process of eliminating genes from the germinal lineage, and, if we are rigorously logical, that the entire process of evolution of life from its beginning on this planet

can be described at least formally as a shedding of gene after gene.

Today it probably will seem more rational to assume simply that impairments are so much more easily produced than new or augmented potentialities through the agency of genic mishaps that the former have thus far overcrowded the field of laboratory observations and stood in the way of finding and studying the more significant positive alterations. If, for example, a gene is momentarily retarded in its process of self-reduplication, a daughter cell must result from which that particular gene is absent. Or if perhaps some slip has occurred in the chemical sequence, and the gene comes to balance on a slightly altered chemical constitution, the gene, by analogy with enzymes, is vastly more likely to suffer an impairment than an augmentation of its catalytic powers. (Johnston and Winchester, 1934.)

A single gene, or gene locus, may be the site for a whole series of genetic alterations, or "mutations," which are all allelomorphic or alternative to each other. It has been found that the mutant forms of organism produced by such a series of allelomorphic genes always differ from each other in degree rather than in kind, showing various levels of accentuation of the respective character, rather than any different assemblage of characters. Shall we draw the conclusion that the gene differences themselves are differences in quantity rather than in kind or nature? Such has actually been inferred, and again has been doubted as a conclusion exceeding the evidence, and by others has been either partially contradicted or even discarded for the completely reverse theory. (Goldschmidt, 1927, 1935a and b; Hammerschlag, 1935; Stern, 1929; Morgan, 1928; Demerec, 1933.)

In this connection one of the most intensively discussed cases of gene modifications is the "Bar" series in *Drosophila*, discovered by Morgan and largely studied in his laboratories. (Hersh, 1934.) This gene locus is found in the X-chromosome and affects the shape and the number of facets in the fruit-fly's eyes.

If we give the symbol B to the original normal gene in the "Bar" locus, there are two major variants known, which can most conveniently be called B_1 (infrabar) and B_2 (bar). Any one of these three acts as allelomorph to either of the others, with the heterozygotes showing more or less intermediate states. But in addition, when cross-over takes place near these genes, they are liable at times to cross unevenly, and in this manner strains have arisen having two of the genes in a single chromosome, either B_1B_1, B_2B_2, B_1B_2 or B_2B_1. The two latter, being quantitatively equal and only different in their relative map positions, do not need separate discussion. Combining two X-chromosomes in one female fly, there results the observed set of genic constitutions with the corresponding average facet numbers characteristic of each (Table 1). In this table the symbol $B:B_1$ means that one of this fly's chromosomes is normal, the other has an infrabar gene; while $B_2:B_1B_2$ indicates one chromosome carrying bar, the other carrying both bar and infrabar.

The first point to note in this table is that the character is determined in the majority of cases by the total list of genes rather than by their manner of grouping. $B_1B_2:B_1B_2$ is the same as $B_1B_1:B_2B_2$, except, of course, that the latter produces offspring in a heterozygous, the former in a homozygous manner. Similarly, $B_2:B_1B_1$ produces the same somatic result as $B_1:B_1B_2$. Only in the combinations $B_1:B_2B_2$ and $B_2:B_1B_2$ does a slight difference appear. One hesitates to judge how much weight to give to this rather small differ-

ence, whether it should count as indicating that differences of location are not always completely without effect, or whether it is merely one of those deviations that will occur in a relatively short string of statistics. In any case the proposition still holds, that nature and count of genes signify more than their positions.

Goldschmidt (1927) has suggested that the numerical relations between these heritable effects are best understood as functions of the cell divisions leading to the formation of the embryonic mother cells of the ommatidia. This is accomplished if we express the numbers as powers of two, to correspond to the number of cell generations. Thus 2^7 or 128, represents 7 generations of cells before facet formation commences. It then becomes obvious that there is a heavy spread of the genic effect between the figures 2^6 and 2^8, indicating that this portion of the curve is modified, perhaps by a time factor, perhaps by a differing grade of physiological susceptibility. This skew in the curve causes B_2 to appear as almost a dominant in relation to B_1, although it is imperfectly recessive to B. From observations of this type Goldschmidt has constructed a powerful argument for interpreting dominance and recessiveness as representing the effects of speed factors and physiological transition phases in the embryological development, rather than qualitative presence and absence. This mode of interpretation is extremely valuable, and there are cases in which its correctness cannot be doubted. Morgan has published convincing evidence that certain of the differences found in the bar series are quantitative, while other differences he is inclined to interpret as qualitative. Goldschmidt on the other hand argues for a quantitative explanation of the entire series, and sees in it a reinforcement of his emphasis upon widespread quantity differences in genes. It is verified by their genetic origin, that double infrabar, B_1B_1 and double bar, B_2B_2 are quantitative increases (doublings) respectively of infrabar and bar. To a biochemist it does not necessarily follow from this that all of the series differs exclusively in the quantities of genic material. Analogies from the endocrine and pharmacological fields indicate that we can ascribe the result equally well to qualitatively modified molecules in some of these genes, leading to speed differences in the physiological catalytic effect. Furthermore, some of the numerical relations almost defy interpretation in terms of a single quantitative variable. This difficulty is best shown in the relations between B_1 and B_2. In the six instances in the tabulation in which B_2 becomes substituted for B_1 and produces a significant result, the differences between the B_1 effect and the B_2 effect are shown in Table 2. A comparison of the fifth entry in this table with the two preceding, and of the fourth entry with the sixth, reveals a

TABLE 1

B : B	779 (normal)	B_2: B_2	68	B_2: B_1B_2	37
B : B_1	716	B : B_1B_2	50	B_2: B_2B_2	36
B : B_2	358	B : B_2B_2	45	B_1B_1: B_1B_2	28
B_1: B_1	320	B_1: B_2B_2	42	B_1B_1: B_2B_2	27
B : B_1B_1	200	B_2: B_1B_1	38	B_1B_2: B_1B_2	27
B_1: B_1B_1	138	B_1: B_1B_2	38	B_1B_2: B_2B_2	24
B_1: B_2	73	B_1B_1: B_1B_1	38	B_2B_2: B_2B_2	24

heavy discrepancy in the amount of alteration occurring when a B_2 is put in place of a B_1. This is suggestive of something qualitatively different in the nature of these genes, since the divergences are difficult to explain if B, B_1, and B_2 are merely different definite quantities of the same gene substance.

The picture presented by the Bar series thus contains features of superlative interest to any who would trace the evolutionary possibilities of the gene mechanism. A taxonomist would, to be sure, view this as a case of retrograde evolution, not of philogenetic progress. But genetically we find a reasonable argument that one gene has been replaced by two, both of which differ from their antecedent in their qualitative chemical constitution, and hence that a genuine progression has occurred from a less complex to a more complex hereditary machinery. It corresponds exactly to the type of change which the evolutionary biologist must believe has occurred millions of times, leading up by successive steps from the simple nuclear mechanism of a primitive organism to the complexities that the higher plants and animals now display. If we are correct in imagining that certain of these gene variations are chemically qualitative, it is only necessary to add the postulate of a regulative principle, such as natural selection, to rationalize the observed evolutionary sequence of progressive adaptation in terms of the mechanics of the chromosomes and the chemical mutability of the particles known as genes. The test of the scientific value of this qualitative chemical interpretation of mutation depends upon its future utility as a framework for further biochemical and genetic research.

An additional argument in favor of the qualitative nature of some mutations can be drawn from the reciprocal relations between groups of mutations that are all referable to one gene locus. So long as mutations are mere alterations in the effective mass of a gene, they will lie in a single, unbranched, ladder-like series for each gene, thus:

$$A \rightleftharpoons B \rightleftharpoons C \rightleftharpoons D \rightleftharpoons E \rightleftharpoons F$$

it being understood that either one or more steps in either direction can be traversed at the moment of mutation. But if chemical alterations of an organic molecule are involved, it is possible to have also a stellate pattern of relationships between the different mutants, in the manner shown in Fig. 1. As an illustration coming from outside the field of genetics, we may cite the stellate manner of interrelation of the substances that cluster about methyl glyoxal in the metabolism of carbohydrate. Some of the alterations are reversible in this case, and others irreversible (Fig. 2). A comparable stellate system of relationships is shown in the cluster of eye-color mutations in *Drosophila*, all occurring at the locus of a single gene. The diagram given here is

TABLE 2

	GENETIC CONSTITUTIONS COMPARED	PHENOTYPES		NUMERICAL DIFFERENCES	CHANGE IN POWER OF 2
(1)	$B : B_1$ *vs.* $B : B_2$	716	358	358	1.0
(2)	$B_1 : B_1$ *vs.* $B_1 : B_2$	320	73	247	2.1
(3)	$B : B_1B_1$ *vs.* $B : B_1B_2$	200	50	150	2.0
(4)	$B_1 : B_1B_1$ *vs.* $B_1 : B_1B_2$	138	38	100	1.85
(5)	$B_1 : B_2$ *vs.* $B_2 : B_2$	73	68	5	0.1
(6)	$B : B_1B_2$ *vs.* $B : B_2B_2$	50	45	5	0.15

combined and adapted from several partial diagrams published by Timoféeff-Ressovsky (1930, 1932, etc.). All mutations that have been observed at this locus have been either to or from the three forms "normal", "eosine", or "white", as here diagrammed (Fig. 3). The arrows indicate the mutations, and the direction of

FIG. 1

mutations, that have been actually observed. Downward arrows represent mutations to a paler eye-color, and upward arrows stand for changes to a more strongly colored form. Statistics on the up-grade mutations are very inadequate, and probably incomplete because of their rarity, less than 9 per cent of the X-ray induced mutations being of this category. It is noteworthy that thus far "eosine" is the only form by way of which a reversion to "normal" has been observed to occur. Obviously the interrelations here shown do not represent a ladder scale, but a sort of triple stellate pattern in which "normal", "eosine" and "white" occupy positions of relative stability which serve also as centers of radiation. This pattern, and also the fact that particular transformations have occurred repeatedly, make very strongly for the hypothesis that a distinctive chemical genetic entity is being transformed into one and another of its nearest related substances. The argument is further fortified when we consider the multiple effects of these genes. In their effect on eye-color we have "white" < "eosine" < "blood" < "normal", but in reference to the color of the kidney of the

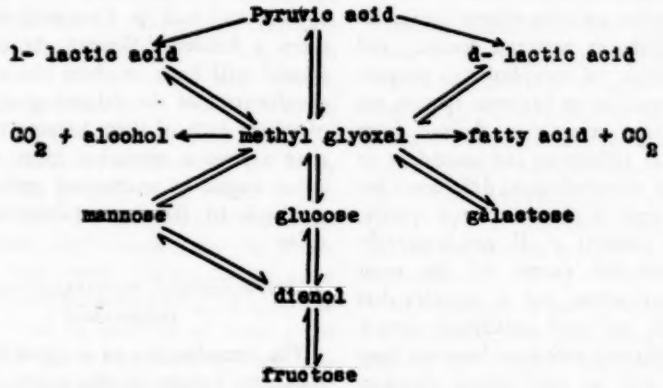

FIG. 2

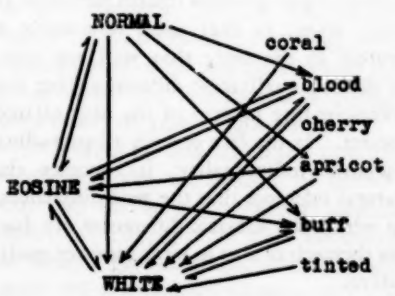

FIG. 3

mutant the first two genes give complete colorlessness, so that "white" = "eosine" < "blood" < "normal". In the

degree of fertility shown in the different classes, "eosine" < "blood" < "white" < "normal". Finally in the vitality of the individual, "white" < "blood" < "eosine" < "normal". Thus the apparent serial arrangement of the same cluster of genes becomes radically different whenever we change the criteria of reference,—a situation better compatible with a qualitative than with a quantitative interpretation of these mutations.

It lies in the background of all these considerations of qualitative versus quantitative differences, that so far as we can tell, the qualitative differences of molecular constitution are everywhere the fundamental differences between species, and that differences of morphology, proportion, or even size as between species are with but rare exceptions, derived from the chemical differences and secondary to them. The morphological differences between the eggs of closely related species, if they are existent at all, are frequently beyond detection except by the most refined observation; yet it appears that serologically we must unfailingly anticipate a qualitative difference between their proteins. And as the organs develop, these immunological specificities of the various organ proteins remain prevalent at every stage, so that there is scarcely a protein in the body that will not react to show a qualitative difference from the corresponding protein of the next related species. In the face of such all-pervading chemical individuality, it becomes the natural inference that the gene differences, to which the somatic differences are due, are themselves also in large measure qualitative.

The quantitative hypothesis that mutations are largely alterations in the amount of active substance carried in a gene, demands obviously that we picture the genes as many-moleculed structures. Conversely the qualitative hypothesis opens the door to the concept of a one-molecule gene, a thought which has been touched upon by various authors (Morgan, 1928; Koltzoff, 1928), but most explicitly brought forward by Demerec. (Demerec, 1933, 1935; Timoféeff-Ressovsky *et al.*, 1935b.) A body consisting of a single molecule, or of a very limited definite number of molecules can, it is pointed out, be most easily imagined to go through these discontinuous, reversible, qualitative alterations. As a future test for this hypothesis Demerec has suggested that the white-eye genes derived respectively from blood, apricot and buff in *Drosophila* ought to carry a hidden difference, because they should still have in them the molecular peculiarities of the diluted gene by way of which each of them became white, so that a reverse mutation from white to color ought by statistical preference to go back to the same particular dilute color.

PHYSIOLOGICAL INTERPRETATION OF DOMINANCE

The introduction of a speed factor, as indicated earlier in this paper, leads toward a modified interpretation of Mendelian alternative inheritance, which fits advantageously into the physiological and biochemical picture of the organism. It can best be developed by reference to some of Goldschmidt's investigations (1927) of the gypsy moth, *Lymantria*. Geographical races of gypsy moths carry Mendelian genes for dark coloration versus extensive light spotting of the caterpillar. In the dark races the pale spots show in newly hatched larvae, but have grown dark by the time of the first molt. The pale races darken only under special experimental conditions, and even then only shortly before pupating. Homozygous intermediate races occur that darken at various

rates during the larval history. Heterozygotes of a light and a dark race simulate the intermediate races, being light enough after the first molt to give the impression that light dominates, yet dark enough after the last larval molt to permit the casual inference that dark dominates. (Goldschmidt, *op. cit.* p. 51–58.) A sound interpretation is only possible in terms of a progressive reaction in which elapsed time and either genic mass or catalytic potency of the genes are influential.

Even more notable are the experiments on intersexes among the species of these gypsy moths. The male-determining and female-determining factors—presumably genes—of this genus differ in a strength among the various races and species. Through hybridation it has been found possible to pit against each other sex chromosomes and autosomes so badly balanced in potency that moths which should be male, by tally of chromosomes, are somatically female, or, in other crosses, vice versa. Less extreme cases give inter-sex mosaics instead of complete reversal, and an analysis of these cases shows that the moths starting with this proper sex, proceed normally to a certain date in their development when the unduly potent genes representing the opposite sex begin to overstep the threshold for controlling the development. Everything produced later than this physiological turning point constitutes the reversed sex part of the mosaic. Each detail is in these cases true to one sex or the other and not a blur of the two. These experiments agree with those on the dark and light colored caterpillars in requiring a temporal progression under the influence of varying masses or potencies of the genes and of the enzymes (oxidases, etc.) whose rate of production they determine. The occurrence of a definite point of time at which the threshold is crossed for a sort of reversal of dominance is extremely instructive, and cannot fail to influence our general concepts of alternative inheritance.

Goldschmidt's hypothesis (1927) that the effect is referable to the speed differences elicited by different masses of competing genes seems entirely acceptable as an hypothesis, and open only to the reservation that different levels of catalytic effectiveness can equally well spring from minor qualitative differences in the molecular structure of the catalyst or gene. The belief shared by Morgan, Goldschmidt, and other authors that recessiveness need not imply the absence of a gene is strongly supported by these intersexes.

GENES AND "ENTWICKLUNGSMECHANIK"

The question how the genes do their work in the organism is anything but a simple one, and any attempt to report its present status would involve an encyclopedic synopsis of the field of Entwicklungsmechanik such as cannot be ventured upon in this paper (Schleip, 1927; Ekman, 1930). Yet at least a few cursory words on the subject are needed in the present connection.

We note first of all that the same gene will not exert the same effect at all times and at all places. The minor but indubitable differences that occur in some of the cases of gene translocation may serve as an illustration (Dubinin and Sidoroff, 1934). This seems to be explained by the consequences of placing genes where they come under the influence of a changed assortment of gene neighbors.

It is obvious that no matter how particularistic the mechanism of heredity may be, the living organism, whether embryo or imago, is no mere assemblage of listable genetic traits, and the fertile germ cell contains no pattern, either actual or symbolic, of the organism that it blossoms into. The gene theory means, rather, that the germ cell nucleus is provided with a numerous but definite assortment of material particles having a

regulatory function, and that these particles initiate the crucial happenings that become determinative of the character and the course of progression of the cellular activities, and thereby eventually of the morphology, physiological make-up and functioning of the developed organism. Every cell in the body carries a complete set, or reasonably near complete, and presumably the whole set is in the main functional in every cell at least as long as the cell is developing, and in all probability as long as it has a functioning nucleus. Each unit of the genic complex stands in active, intimate relationship to other units present in the same nucleus, to cytoplasm, and to more remote physical and chemical influences, all of which items, both intracellular and extracellular, are in a sense environmental to it. In the different tissues all factors lying outside the nucleus progressively become highly divergent, as the individual develops. Thus there must come to pass a perpetual shifting of the relative effectiveness, and even of the qualitative effect of the action emanating from any one gene, due to the changing medium in which it occurs. In one ontogenetic environment the climax effect of a particular gene may come early, in another it may come later, thereby inducing a sharply different end result (J. B. S. Haldane, 1932). The progressive specialization of the cytoplasm to which we have just alluded is described in different terms by Just (1936).

This author puts greater emphasis on the cytoplasm as a prime seat of the manifold vital processes. He points out that the nucleus stands in a sort of parasitic relation to the cytoplasm, from which it obtains its metabolic wherewithal. In doing so, he argues, it may easily strip the cytoplasm of the material anlage for one and another physiological potentiality, thus bringing about a specialization of the cells. The genes are accordingly interpreted as regulators of the stripping (and specializing) process, while the cytoplasm is described as progressing—or degenerating—from a state of multiple potentialities and abundant endowment to a state of one-sided specialization. Although this was offered as a substitute theory, there does not seem to be any *prima facie* incompatibility between the mechanism it suggests and the more orthodox picture of gene action. Conceivably one gene might work in both ways. Just's hypothesis has the merit of agreeing best with the usual interpretation of the Entwicklungsmechanik of tissue differentiation.

In the processes of ontogeny it is constantly observed that the different parts of an organism not merely keep step with each other chronologically, but are actually influenced by each other so that their various differentiations are actively held in step even under somewhat abnormal experimental conditions. This "integrative" effect freely oversteps the cell boundaries and necessitates the handling of many embryological problems from the standpoints of tissue masses, chemodifferentiation (Julian Huxley, 1932), organizer substances (Spemann, 1927; Needham, 1931 and Needham *et al.*, 1934), metabolic gradients (Child, 1915), etc., in place of cell lineage and cellular mosaic. In cases of regeneration, which we may look upon as a sort of belated embryology, cell masses frequently develop in directions totally contrary to the implications of their cell lineage. At first glance these findings seem at variance with any particularistic theory of heredity, but the answer has been made that the gene theory differs from all other particularistic theories of the past in such a way as to give it an escape from this criticism (Goldschmidt, 1927). This is partly because it ascribes in the main similar sets of genes to all cells of the developing organism, making it conceivable for cells with dissimilar embryonic history to replace each other, and partly because it must explain the specialization of organs and tissues as due to the accumulation of differential physiological conditions exterior to the

genes, viz. to the above-mentioned chemo-differentiation, hormone influences, metabolic gradients, etc. Its own findings and postulates force it, that is, to become what numerous authors have protested was necessary in any good theory of heredity—physiological in viewpoint.

THE GENE PICTURE IN MAN

The study of mammalian and human heredity involves a number of difficulties, not the least of which is the slow rate at which data can be accumulated. It would take several thousand years to assemble in man such experimental pedigrees as have already been obtained for fruit-flies. Hence, unless we would leave the discussion of human heredity to our remote successors in science, we shall have to proceed to a considerable extent in terms of analogy and comparison.

Our count of chromosomes has been reported variously from different laboratories.

The haploid number, found in female-producing sperms and in unfertilized ova is reported to be either 24 or 12 chromosomes. This probably represents a difference in the state of aggregation rather than a direct total discrepancy in the biological material, since the universal inter-varietal fertility of our species is evidence of a fairly constant total count and arrangement of genes in man. The male-producing sperms are credited by different authors with either 23, 24, or 12 chromosomes, the chief disagreement here being as to the presence or absence of a small Y-chromosome. (Wieman, 1917; Winiwarter, 1910–11; Painter, 1923, 1924.) The preponderant evidence is that the Y exists, but functions genetically as very nearly a blank. (Oguma and Kihara, 1923.) The male diploid count of effective chromosomes is accordingly 47, 48, or 24, as against a female diploid count of 48 or 24. (Schachow, 1926.)

Data have been accumulated sufficient to show that the Mendelian rules apply in principle to long lists of human traits. Frequently, however, the same measurable trait is under the influence of genes in different chromosomes, with a complicating effect. The details of these extensive studies hardly belong in this paper, although they are eugenically extremely important, and serve to demonstrate the likeness between heredity in man and in other species. It is of especial interest that in addition to the usual bodily characteristics various neurological peculiarities and even certain traits of the mental constitution have been reported as heritable in the Mendelian manner.

There is thus far only a very limited list of gene linkages known in man. This is because a zygote count of 47 or 48 chromosomes means that only by special luck or industrious search will observers obtain statistics on two genetic characters carried in the same chromosome in the same human pedigree. The best existing series of this sort was collected by Bell and Haldane (1936) for two X-chromosome characters, hemophilia and color-blindness. (Cf. also Hogben and Pollack, 1935; Yorshis and Gottlieb, 1934; Shōji and Ninoyu, 1935; and Zieve *et al.*, 1936.) They find this linkage to be very pronounced, 17 test cases giving 16 examples of linkage and one doubtful example of cross-over.

To the genealogically inclined it will be bad news that no person is effectively descended from all his reputed ancestors. Our 47 or 48 chromosomes come 24 from the mother, the rest from the father; but from grandparents and earlier ancestors the derivation follows the mathematical rules of "pure chance." Discounting Y as a "blank", a paternal grandparent of a boy has one chance in 8,388,608 (i.e. $1:2^{23}$) of failing to contribute a significant chromosome to the grandson. A maternal grandparent has correspondingly one such chance in 16,777,216 (i.e. $1:2^{24}$). A girl's paternal grandmother is sure to have provided her with an X-chromosome, and has one chance in 8,388,608 that this is

the only one. The chances for other grandparents are the same as in the case of the boy.

There is a 46 per cent likelihood that a particular maternal grandparent supplies between 11 and 13 of the child's chromosomes (inclusive); a 69 per cent likelihood that the share runs between 10 and 14; and less than one chance in 300 that his share is 5 chromosomes or less.

The statistical case for great-grandparents is far less favorable. If we disregard the problem of "cross-over", we find there is 1 chance in 996 that in a particular infant of unidentified sex the great-grandparent in question is not represented by any chromosome. Adding one more generation, and there are better than even chances (509:491) that not more than 15 of the great-great-grandparents have any of their chromosomes present in a specified descendant of unidentified sex. The phenomenon of cross-over will modify all these figures on the distribution of genetic material to a degree not as yet determinable, but always in the direction of wider representation, and ever-increasingly as the generations progress. The maximum limit of effective ancestors is our quota of genes, whatever that may be. In no case will the figure be high enough to provide more than a gambler's chance that an ancestor who, let us say, brought fame to the family at the battle of Crécy, has actually contributed a single gene to a particular descendant living today for there cannot possibly be enough genes to share around among the half million ancestors of that date.

An especial contribution from the human realm is the comparative study of "identical" twins, that is to say, of two individuals originating from a single fertilized ovum, and hence provided with identical assortments of genes (Rife, 1933). From such data we should learn just what limit there is to the precision of hereditary control. On the physical or ordinary somatic side a remarkably close correspondence has been recorded between twins of this type. For example it is demonstrated that in such minor items as the fingerprint patterns the genes are in some way responsible for the pattern type, a matter in which ordinary brothers and sisters show great diversity, but that the actual count of the epidermal ridges by which the pattern is executed is not closely controlled by genes. (MacArthur and MacArthur, 1937.) In most cases the similarity of pattern between corresponding hands of identical twins is about comparable to that found between the two hands of one ordinary individual.

When we turn to mental development and social characteristics the evidence becomes blurred by uncontrollable external complications. The question is here twofold; (1) How similar are individuals in which both the genes and the environment are alike? and (2) How similar will individuals remain if their identical hereditary traits are subjected to contrasted environments? The answer to the second question shows that although the individuals become different, the degree of difference is appreciably less than is statistically expected for ordinary brothers with contrasted environments. For the first question, however, a clear answer is almost out of the range of possibility, because identical environments cannot exist for two human individuals, even in the same family surroundings. It is impossible even for twins to remain in reciprocally identical social relations to each other. For example in the matter of leadership in their mutual relations, either one will become habitually the leader, or their leadership will become distributed between them in a topical manner, eventually hardening into fixed

habits and traits of character. Their own desires to be alike, or to be unlike, will also skew the results. Hence there is need for very discriminating statistical study on a larger scale than has yet been attainable.

GENES IN INTER-SPECIES STERILITY AND DIVERGENT EVOLUTION

During the evolutionary discussions of the 19th century it was pointed out by J. T. Gulick (1872, 1887, see also 1905; and Addison Gulick, 1932) by Romanes (1886, 1897) and by Karl Semper (1881) that inter-species sterility constitutes one of the major conditions for the progressive evolutionary divergence of species from each other. Semper commented (edition of 1889) that geographical isolation, which plays such a prominent rôle among land and coastal species, is virtually non-existent in the whole realm of pelagic plants and animals, so that here the entire process of species formation is conditional upon physiological isolation. More recent authors have called renewed attention to the evolutionary significance of sterility between groups (Sewall Wright, 1931; Robson, 1928; Robson and Richards, 1936.)

Genetics and serum biochemistry suggest several possible roads by which a condition of inter-species sterility may come to pass:

(1) The translocation of groups of genes into chromosomes not homologous to the ones originally occupied. Where one such accident of translocation has occurred, offspring are produced, only ½ of whose germ cells are viable. With multiple translocations it has been found that only $(\frac{1}{2})^n$ of the germ cells are viable, n being the number of translocations. If n becomes sufficiently large this will virtually establish an inter-species sterile-mule condition, the state to which J. T. Gulick gave the name "Segregate fecundity." Its distinctive characteristic is that the fertilization of the ovum is accomplished, but adequately fertile off-spring do not result.

(2) Closely related to the last, if there are extensive inversions and other rearrangements of the gene chains even without their transfer to other chromosomes, there results an accumulated embarrassment of the process of synapsis in hybrids, that may contribute toward establishing a sterile-mule condition.

(3) Another form of segregate fecundity has been reported, which is dissimilar for reciprocal crosses, A × B being fertile, yet B × A producing only sterile results, at least for one of the sexes. This is the situation where translocation of essential genes has occurred between the X- and the Y-chromosomes.

(4) Theoretically a situation should be found where a gene mutation sets up a new serological condition that makes the genital tract fluids or the germ-cell sap incompatible with those of the old stock, thus preventing any fertilization from occurring. Conditions more or less of this sort are thought to occur in nature, and were named "Cross impotence" by J. T. Gulick. This differs from segregate fecundity in that the ovum is untouched, and hence is not prevented from becoming fertilized by a more appropriate male. No mutation of this sort has been observed occurring in the laboratory, and the genetic background for this supposed condition is still unknown. It is not necessary to assume that such a serological change need depend on the mutation of any great number of genes, especially as we note that the mutations whereby certain human bloods become agglutinative of other bloods are referable to the alteration of one or another among two or three genes. In view of the manifold qualitative differences of protein constitution detected even in closely related species, evidence for inter-sterility of this sort as a result of gene mutations ought to be discoverable.

(5) Another angle of this problem may be seen in what are known as fertility and viability genes, and especially the possibility that there are genes that may function as conditional lethals. Each of these categories admits of situations that may increase the sterility between dissimilarly constituted germ plasms. The conditional lethal effect may be illustrated from the fruit-fly mutations known as eyeless, bent-wing, and shaven. These all occur in the small chromosome, and all produce viable results both in the heterozygous and homozygous conditions. If, however, a fly happens to be haploid for the small chromosome, the presence of any of these recessive genes becomes lethal. It is but a short extension of this to suppose that one species may have genes that are lethal unless offset by certain other genes of the same species, or liable to set up lethal processes if inappropriately combined with certain alien genes. Many facts of interspecies sterility would fit into

some such genetic pattern, but it is evident that in a field where so many alternative hypotheses can be suggested the real need is for fuller experimental data.

Dobzhansky cites cases in the genus *Drosophila* which he interprets as belonging to this category of conditional lethals, although they give a first impression of being in category 3 on account of a preponderant lethal effect in the male sex. (Dobzhansky and Boche, 1933; Dobzhansky, 1937.)

Thus with the progress of years we find a striking reinforcement of the scientific cogency of the theory which Romanes and J. T. Gulick championed; namely that a physiological barrier between two otherwise hardly distinguishable stocks may occur frequently, and must have the effect of initiating a train of divergent evolution. But today it would be restated in the modern wording that it is possible for two forms very nearly alike in their total assortment of genes to be mutually sterile by virtue of one or two trifling mutations, or even by relocation of genes without any alteration of total content, with a result that most initiate a divergent evolution.

OTHER EVOLUTIONARY IMPLICATIONS

The gene theory, with its picture of a not unlimited quota of genes, each of which is subject to such mutations only as are permitted by its status as an organic chemical molecule (or small aggregate of molecules) gives us a very different mental image of what happens when a species undergoes alteration than would be gathered from any of the earlier theories of heredity. We note:

1. Since the primary origin of gene mutations is from physicochemical causes, and their actual nature must be that of a chemically definable alteration of the gene substance, we find that from the biological viewpoint these mutations have a decidedly unteleological and even inconsequential flavor. Genetic adaptation apparently has to do with what the evolutionary forces can accomplish out of these mutations, rather than with the primary nature of the newly produced genes.

2. Mutation possibilities are restricted to such changes as can occur and be perpetuated in the chemical configuration or numerical readjustment of genes already present. From this it follows that:

(a) The *directions* of possible mutations are not perfectly *ad libitum*, but are limited to changes in such directions as can find expression in the gene complex actually on hand.

(b) Variation in any one direction cannot be subdivided into infinitesimal gradations. Chemical changes in physical particles that approach molecular dimensions are necessarily discontinuous.

(c) The total ultimate *extent* of variation in any one direction is theoretically subject to the limitations of what can be expressed in a finite number of genes, each with finite variability. This principle should eventually become important to students of evolution, although it would seem audacious to invoke it at present in connection with any specified problem in evolution.

3. The experimental fact of linkage covers two types of phenomena, viz. effects referable to different genes located in the same chromosome, and those referable to a single gene at work in different tissues of the body. The former has only a transient significance, because of the possibility of cross-over; but the multiple effects, perhaps seemingly unrelated, which come from the several influences of one gene, are a real limitation upon the path of evolution. Certain color genes are, for example, impossible to fix, because in addition to their color effects they function as recessive lethals. Considerations of this sort may eventually supply explanations for many otherwise unaccountable vagaries of evolution.

4. Despite the finite number of genes and of gene mutations, the factorial system of inheritance provides possibilities for an incredibly copious diversity of individual heredity, which serves as the crude material upon which natural selection can work (Sewall Wright, 1931). On the principle that each new mutation doubles the number of theoretically available mutation combinations, the known mutations in *Drosophila*, more than 400 in number, should permit of 2^{400} (= to about 10^{120}) different conceivable combinations. The unimaginable magnitude of this figure is illustrated by the fact that the total mass of the astronomically known universe expressed in milligrams has only been estimated to reach the figure 10.[60]

5. In the light of the gene theory biparental propagation is found to be much more conducive to individual diversity than might have been supposed, and this effect is much less impaired by temporary inbreeding than other theories would lead one to anticipate. The chances are infinitely against identity of the genes carried by two brothers in a family derived from an ordinary population. Formulae developed by Sewall Wright (1922, 1923) indicate that starting with outbred stock and disregarding linkage, with its complications, it requires three generations of brother-sister breeding to accomplish statistically a 50 per cent diminution of heterozygousness among the genes retained, and that ten such generations will only diminish the heterozygousness by 89 per cent. Such persistency of the heterozygous condition has never before been suspected, especially as it has long since been recognized as a commonplace that 10 generations of hermaphroditic self-fertilization (as in plants) will give not less than a 99.9 per cent elimination of heterozygousness.

6. The stability and persistence of genes saves a character from being entirely lost through outbreeding. Even a recessive may live on unsuspected, only to reappear when a mating, perhaps between remote kin, brings it to light again. This principle can do for evolution some of the things for which isolation had to be invoked in the pre-Mendelian days. (Robson, 1928; Robson and Richards, 1936.)

7. The stability of genes also augments the effect producible by isolation, since it provides an accumulation of hereditary potentialities which may be brought to light by isolation and its resulting inbreeding.

8. Any mutation that can occur once can occur again, in the sense that if one molecule of substance *A* has transmuted into substance *B* we may well anticipate that other molecules of *A* will undergo the same transmutation. This contrasts with the old viewpoint, according to which small variations were indefinitely intergraded in all directions, and larger "sports" were too much in the nature of freaks to permit the likelihood of their repetition. It may be inferred that even without the inbreeding of relatives, a new homozogous character may become established through the mating of diverse stocks in both of which the new gene has independently originated.

9. The significance and effectiveness of natural selection can now be tested by appropriately ordered experiments, and appraised by pertinent mathematical analysis. This possibility had to wait upon the acquisition of reasonably correct formulae for the mode of action of heredity and mutation. (Fisher, 1930; Sewall Wright, 1931, 1934, 1937; Haldane, 1936.)

(To be concluded)

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THE SEX RATIO IN MULES AND OTHER HYBRID MAMMALS

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INTRODUCTION

A SURVEY of the literature discloses that the relative proportion of the sexes in mammals began to receive the attention of observers about the middle of the 18th century, and that the sexes among hybrids were the first to be recorded, although the numbers were very small. The meagerness of such records is attested by Darwin (1874), who stated, that so far as known to him no one had paid attention to the relative numbers of the sexes throughout the animal kingdom. Apparently this statement by Darwin aroused the interest of investigators, since numerous observations on the proportion of the sexes were recorded for various species of mammals and birds between 1880 and 1910 and much discussion entered the literature concerning sex ratios and the determination of sex.

Buffon (1791) was one of the first to call attention to the proportion of the sexes in hybrids. He expressed the opinion that an excess of males occurred. Colonel Hamilton Smith (1841) stated, that it is observed in general that males are much more abundant than females in hybrids and that the fact is equally true in mules, where the males occur in the proportion of two or three to one female. Gadeau (1899) also stated, that the results of hybridization are much oftener males than females, and that male offspring are more numerous in proportion to the specific distance between the two parents. Suchetet (1897) expressed the opinion that in hybrid birds males are more numerous than females and cited numerous observers in support of this opinion.

In the early discussions pertaining to the sexes of hybrids the writers frequently neglected to distinguish between mammals and birds. This, together with the fact that males do occur more numerously than females among hybrid birds, may account in part for the view that males are in excess in hybrid mammals as well.

Haldane (1922) gave a discussion of the sex ratio in hybrids based only on cases bred in captivity and for which more than 10 offspring were raised. Furthermore he included only cases where one sex was absent, sterile or the sex ratio was markedly disturbed. From this study he advanced the hypothesis that when in the F_1 offspring of a cross between two animal species or races, one sex is absent, rare or sterile, it is the heterogametic sex.

Because of the need for a more extensive investigation than has heretofore been made of the proportion of the sexes in hybrid mammals the present study was initiated. The attempt has been made to assemble as many published records of the sexes of hybrid mammals as are available, and to collect as large a number as possible of actual birth records of the sexes for one or more interspecific hybrids. Furthermore, it is desired to make a comparison

of the sex ratios found for hybrids with the normal sex ratio for the species whereever possible, in order to determine with reasonable precision how much hybridization actually influences the proportion of the sexes. Since the mule is so commonly produced for commercial purposes this hybrid appeared to offer the best possibility for securing reasonably large numbers.

At the outset it was hoped that the desired records might be obtained from farms and ranches which had been producing large numbers of mules. However, a survey of the mule-producing territory in the United States revealed that records of the sexes had not been kept by those engaged in mule production. Furthermore, that ranches that had formerly made a business of producing mules had largely discontinued the practice. Jacks are now owned by individuals and the mules are produced by farmers who breed one or more mares yearly to a neighboring jack stood for this purpose. Since farmers seldom make records of the sexes of colts and produce only a small number of mules individually, some plan had to be followed which would yield foaling records from a large number of farms. A plan for obtaining records through the cooperation of farmers and jack owners was outlined and presented to the Committee on Animal Breeding for the National Research Council for suggestions. This committee approved the plan and urged that the investigation be made. Acknowledgment is made for suggestions and encouragement received from the committee.

The study was begun at the University of Wisconsin, and continued at Oklahoma Agricultural and Mechanical College. Doctor L. J. Cole proposed the problem and has given helpful suggestions and assistance in obtaining the data and preparing the manuscript. Professor W. L. Blizzard has made it possible to continue the study. Grateful acknowledgment is made to jack owners, farmers, county agents, vocational agricultural teachers and animal husbandrymen who have cooperated in obtaining the records.

RESULTS

1. Sex ratio of the horse

Darwin (1874) was apparently the first to report the proportion of the sexes for the horse. He obtained 25,560 records from the "Racing Calendar" for the thoroughbred and found a ratio of 99.7 males to 100 females. Wilckens (1886) and Düsing (1887, 1888) gave reports based on records secured in Germany from government horse breeding farms and private records, which had been recorded for a number of years. Wilckens' material included more than 16,000 records and shows a sex ratio of 97.3. Düsings' earlier report contained more than 800,000 records with a sex ratio of 98.31. His later report includes more than a million records and gives a sex ratio of 98.75 or 49.69 ± 0.002 per cent males. The earlier ratio given by Düsing appears to be quoted more often than any of the others in discussions of sex ratios.

Kisslowsky (1932) points out that Cornevin (1890) found the sex ratio of the horse to be 101, and further that Baldassare (1896) reported it to be 102. He also cites von Oettingen (1921), who maintains that more male than female horse foals are born, averaging approximately 106 males to 100 females. Calder (1927) is cited by Crew and Smith (1930) for his study of the sex ratio of the horse, based on the Clydesdale stud-book of England. He found the sex ratio to be equality.

Since the earlier records gave an excess

of females and later records showed an equality of the sexes or a slight excess of males, some doubt appears to be justified concerning the dependability of the sex ratios given. It is difficult to know how complete the records were. McPhee (1927) has shown that swine herd-book records are not sufficiently accurate for the establishment of reliable sex ratios. There is no apparent reason to indicate any greater degree of reliability of stud-book records for this purpose. Therefore, a need for complete foaling records was realized in order to make a comparison with the sex ratio obtained for the mule.

In attempting to find foaling records contact was made with the Colleges of Agriculture in the United States and records were obtained through the cooperation of eleven institutions and one horse breeding farm, as follows: Univ. Ill.; Ia. Sta. Coll.; Kans. Sta. Coll.; Univ. Minn.; Univ. Mo.; Cornell Univ.; Ohio Sta. Univ.; Okla. A. M. Coll.; Tex. A. M. Coll.; Univ. Wis.; Univ. Wyo.; Prairie Farms, Alicia, Mich. (courtesy J. F. Ziegler, Mgr.). Grateful acknowledgment is given for these records furnished by those in charge at the different institutions. A summary of the records shows; live-born foals (single births) 626 males and 581 females; still-born 20 males and 8 females; sets of twins, 3 pairs of males, 4 pairs of male and female, and 7 pairs for which sex was not recorded. The total sexes recorded are accordingly 656 males to 593 females, the sex ratio being 110.6 or 52.52 ± 0.95 per cent males. The sex ratio of the live-born foals is slightly lower, being 107.7 or 51.8 per cent males. At this point it should be said that the probable error, rather than the standard error, has been used throughout this paper.

Efforts to obtain information as to the normal sex ratio of the ass have been unsuccessful.

2. *Sex ratio of hybrid Equidae*

a. Hybrids previously reported

In Table 1 a summary is given of the sexes of the hybrid Equidae which were found recorded. Although the numbers are small females are in excess, giving a sex ratio of 44.74 ± 5.45 per cent males. Ewart (1899) reported 9 hybrid foals by a zebra stallion mated to mares, but sex was given for only 8. In a later discussion (1910a) he called attention to 16 hybrids of this cross but the sexes of these were not mentioned. Pocock (1911) described a hybrid between a wild ass and a quagga mare but the sex was not recorded.

In response to our inquiry for information concerning the sexes of hinnies (progeny from female ass, or jennet, mated to stallion), Mr. R. Lynch, of the Department of Agriculture, Dublin, Ireland (In litt. 1932), states:

> Regarding the sex ratio among Hinnies it would appear from the reports furnished by our local officers after examination of upwards of 1,000 animals that females predominate. The percentage works out at 58% females and 42% males. On the other hand in mules the number of males appears in excess of females, the proportion being 61% males and 39% females.

Statistics on the sexes of mules in Spain for 1928 were secured from the American Consular Service (In litt. 1932, courtesy Mr. Curtis C. Jordan). According to the information obtained there were 458,542 males and 410,106 females, a sex ratio of 52.79 per cent males. Since these reports do not represent foaling records, and may therefore be selective, they are not considered to be sufficiently reliable for use.

According to Lotsy (1922), Gottschling reported 196 matings of female asses to a stallion, from which only 10 colts were born. From 25 matings of female asses

to a zebra stallion 3 foals were born. In 119 matings of mares to jacks 51 became pregnant. The sexes of these hybrids were not given but it was mentioned that all of them were infertile. Tegetmeier and Sutherland (1895) stated, that 3 zebra hinnies from a female zebra and by small stallions were seen in Sir Henry Meux Park, but again sex is not given.

b. The mule

(1) Plan and source of data

The plan followed in securing the data herein reported for the mule was to make contact with reliable persons who were sufficiently interested in such a study to assist in getting the records. County agricultural agents and vocational agricultural teachers were approached on personal acquaintance or on recommendation by animal husbandrymen, and the coöperation of several was enlisted. Blank forms were prepared and placed in the hands of those who had agreed to assist; they distributed the blanks to reliable jack owners, who supplied the data from foaling records kept for the service of their jacks. In a few cases the coöperators secured service records for one or more jacks and followed up each mare and obtained a report of the sexes from the mare owners.

The plan was followed for two years. By this time a number of jack owners had become sufficiently interested to volunteer their services in supplying records directly, and blanks were sent to them, which they returned at the end of the foaling season. Complete records were obtained in all but a few cases. The few incomplete reports were due to the sale of mares by the owner following service, so that contact was broken; also a few mares aborted while in pasture and the sex of the foal was not observed. An appeal was also made to the Agricultural Colleges for records of the sexes of mule foals but only two had such records.

TABLE 1

Summary of the sexes of hybrid Equidae (exclusive of the mule) reported by various authors

MALE	FEMALE	M	F	AUTHORITY
Zebra	× Mare	3	5	Ewart (1899)
Zebra	× Ass	6	5	Riley (1909)
Zebra	× Mare	3	5	Roberts (1919)
Zebra	× Mare	1	0	Sclater (1903)
Zebra	× Mare	1	0	Hesse (1899)
Ass	× Zebra	0	1	Pocock (1911)
Quagga	× Mare	0	1	Ewart (1910) Lord Morton Case
Horse	× Ass	2	2	Anonymous (1890)
Horse	× Zebra	1	0	Iwanoff (1905)
Shetland	× Bergzebra	0	1	Dr. Lutz Heck (*In litt.* 1932)
Riesenesel	× Chap. Zebra	0	1	Dr. Lutz Heck (*In litt.* 1932)
Przewalskii (wild)	× Zebra	1	0	Gunali (1933)
		18	21	

(2) Sex ratio of the mule

The sex records collected for mule foals by the plan described, including the foaling seasons of 1929 to 1932, are shown in Table 2. For the 1416 mules (627 males and 789 females) sired by 98 jacks the sex ratio is 44.28 ± 0.89 per cent males (79.5 males:100 females). This is practically the same ratio as obtained for all other

equine hybrids as summarized in Table I. Records for an additional 138 mules (74 males and 64 females) were obtained from the Mississippi Agricultural Experiment Station through the courtesy of Professor H. H. Leveck. However, the sexes for these were not recorded until the mules were one or more years old. The number of each sex that died or was sold before the records were taken on this group is not known. Therefore, these records are excluded from the table.

(3) Sex ratio of mules by different jacks

The percentage of males sired by each jack was calculated and the results for jacks that sired 3 or more foals are presented is Figure 1. The distribution shown in the figure indicates that these data are a random sample of jacks and that there is no evident tendency for certain jacks as compared with others to sire a preponderance of one sex. The fact that some jacks have produced foals all of one sex is satisfactorily accounted for on a basis of chance.

TABLE 2

Sexes of mules reported from different states

STATE	NO. OF JACKS	LIVE-BORN FOALS		STILL-BORN FOALS		SETS OF TWINS		
		M	F	M	F	mm	mf	ff
Ala. (1)	2	22	17	1	0	0	0	0
Iowa	5	6	9	0	1	0	1	0
Ill.	14	136	174	16	11	0	0	1*
Kans.	4	30	41	5	4	0	1	0
La. (2)	3	17	10	0	0	0	0	0
Mo. (3)	8	46	74	5	9	0	0	1
N. C. (4)	6	26	25	0	0	0	0	0
Okla.	52	269	341	6	6	0	2	4**
Tenn. (5)	4	37	49	1	2	0	0	0
Total	98	589	740	34	33	0	4	6

1. Through cooperation of Mr. Matthew Woods, Tuskegee, Ala.
2. La. Agr. Exp. Sta. courtesy Dr. Chas. I. Bray.
3. Through cooperation of Prof. E. A. Trowbridge.
4. Through cooperation Mr. Chas. A. Sheffield.
5. Through cooperation of Dr. Moses Jacob.

* Still-born twins.

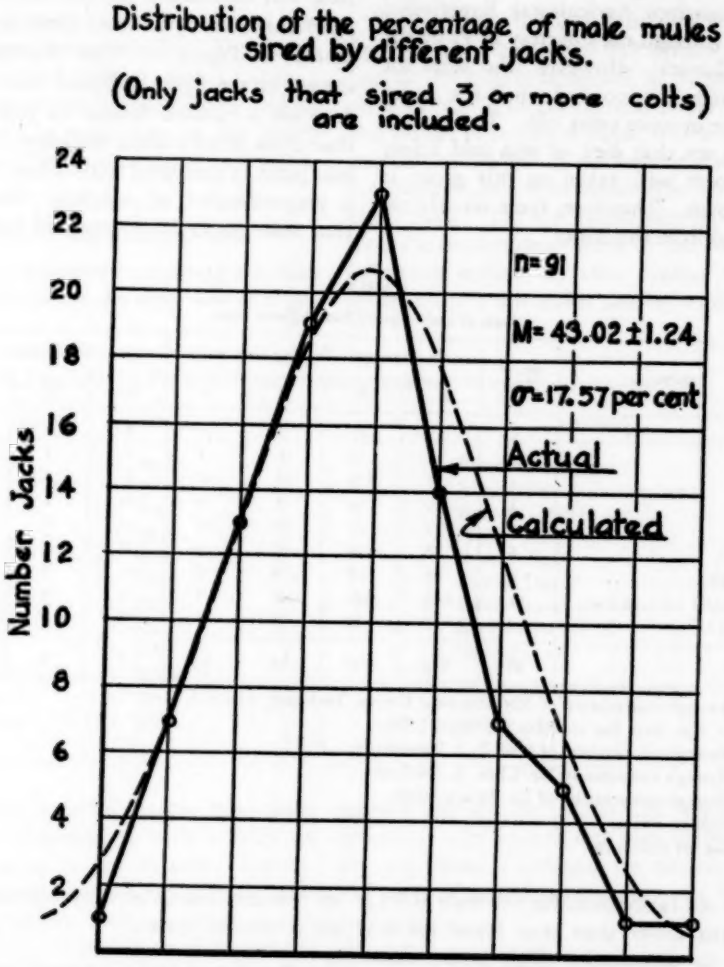
** One set still-born.

The sex ratio found for the mule is 8.24 per cent lower than that found for the horse in this study. This is 6.3 times greater than the probable error of the difference. Even when the sex ratio for the horse reported by Düsing (1887) is used the sex ratio for the mule is 5.41 per cent lower than for the horse. This difference is 6 times greater than its probable error. Accordingly, in either case, the difference is distinctly significant.

(4) Fertility of jacks compared with that of stallions

Reports for the number of mares bred to 75 different jacks were complete for both breeding and foaling data. Only 51.9 per cent of these mares produced foals. This is 9.9 per cent greater than Gottschling observed according to Lotsy

(1922). He found that only 42 per cent of the mares bred to jacks became pregnant. Darwin (1874) mentions that data furnished to him by Tegetmeier from the "Racing Calendar" show that two-thirds (66 per cent) of the matings for the thoroughbred were successful. Hammond (1914) mentions that the Royal Commission on horse-breeding found that in mares about 40 per cent of those selected for breeding failed to produce foals. Kisslowsky (1932b), using selected stud-



book records for a group of 83 stallions and 83 of their sons (Cremer's data), found that 61.38 per cent of the matings for the fathers and 57.7 per cent of the matings for the sons resulted in conceptions. Sanders (1926) found the mean percentage of fertile matings for more than 1800 stallions to be 53.54 per cent. It is of interest, however, that one group of stallions gave a mean fertility of 57.37 per cent for the period 1887 to 1910, and for another group covering the period from 1911 to 1924 the mean fertility was only 51.53 per cent. A fairly constant decline in fertility occurred from approximately 61 per cent at first to 50 per cent at the end in Sanders' study. Gottschling (Lotsy, 1922) found that only 5.1 per cent of the matings of stallions to jennets were fertile, while 12 per cent of the matings of zebras and jennets were fertile. On a basis of these reports it appears that the percentage of fertile matings may be lower for jacks than for stallions when both are mated to mares. This might be accounted for in part by a lower sex ratio among mule than among horse colts. However, two other reasons appear as possibilities; (1) the advanced age of the mares bred to this group of jacks in this period, (2) mares are often put to a jack after they have repeatedly failed to conceive to the service of a stallion. Some of these mares are believed to be sterile.

(5) Still-births and twins

The records show (Table 2) that 5.01 per cent of the mules were still-born, and two pairs of female twins were among these. Although more females than males were still-born the difference is insignificant. Ten sets of twins were reported, which is a frequency of only 0.71 per cent of the total births. No case was reported for which the twins were both males. Sex was recorded for 40 still-born horse foals; 31 of these were males, a sex ratio of 77.5 per cent. There were 14 still-born horse foals (twins) for which sex was undetermined. Thus 54 horse foals were still-born. This is 4.28 per cent of the 1263 foals. It is also of interest that 64.3 per cent of the horse twins were still-born. The frequency of twin births is 1.12 per cent, which is exactly the frequency to be expected according to Richter (1926).

3. Sex ratio in the Bovidae

Johansson (1932a) has recently made an exhaustive study of the normal sex ratio from herd records in the Swedish and Finish breeds of cattle and found it to be 51.52 per cent males. This is in close agreement with the ratio commonly reported for cattle (*Bos taurus*).

Unpublished calving records of the sexes for 8646 head of *Bos indicus* were obtained through the courtesy of Colonel Matson, O.B.E., I.A. Civil Lines, Jubbulpore, India, and for 124 head from the Louisiana Agricultural Experiment Station (through courtesy of Dr. Chas. I. Bray). The sex ratio of the latter is 51.6 per cent males. Combining the two sets of records gives a total of 4466 males and 4304 females, or a ratio of 50.92 ± 0.36 per cent males. This is very close to the normal ratio for domestic cattle (*Bos taurus*).

a. Sex ratio of hybrids

The figures shown in Table 3 are a summary of the sexes of F_1 hybrids which have been found for the Bovidae. For the cross of bison and cattle 19 males and 62 females are shown, a sex ratio of 23.5 ± 3.18 per cent males. It is frequently mentioned by those who have reported this cross that much difficulty arises due to the presence of excessive amniotic fluid in the female cow carrying the hybrid calf. Furthermore, that the hump on the male calf increases calving difficulties for *Bos*

taurus females. Boyd (1908, 1914) reported 6 males and 39 females from crossing American bison bulls on domestic cows. Three of the males died at birth and one lived only a day. The two surviving males proved to be sterile. Fifteen of the females failed to produce calves although mated to both bison and domestic bulls. Only 3 female cattalo, as the hybrids are called, were reliable breeders. Hybrid females mated to bison bulls produced 12 males and 18 females. Boyd also points out that 63 cows mated to bison bulls aborted, but he does not report the sexes of the calves. Deakin, Muir and Smith (1935) reported 11 abortions and 10 still births for which the sexes of the calves are not given. However, the two males shown in Table 3 by these authors are reported as live-born.

TABLE 3

Summary of the sexes of hybrid Bovidae

MALE	FEMALE	M.	F.	AUTHORITY
Amer. bison	× Dom. cow	6	39	Boyd (1908, 1914)
Amer. bison	× Dom. cow	1	3	Kühn (From Lotsy 1922)
Amer. bison	× Dom. cow	0	3	Rothwell (1924)
Amer. bison	× Dom. cow	2	3	Iwanow and Philiptschenko (1916)
Amer. bison	× Dom. cow	2	4	Deakin *et al.* (1935)
Dom. bull	× Bison cow	7	7	Deakin *et al.* (1935)
Europ. bison	× Dom. cow	1	3	Iwanow and Philiptschenko (1916)
		19	62	(23.5 ± 3.18)
Banting	× Zebu	1	0	Nathusius (1911b
Yak	× Sanga	1	0	Nathusius (1911b)
Gayal	× Sanga	0	1	Nathusius (1911b)
Zebu	× Yak	1	2	Zawadowsky (1931)
Yak	× Zebu	1	0	Mitchell (1849)
Yak	× Dom. cow	9	10	Kühn (Lotsy 1922)
Yak	× Dom. cow	1	5	Rothwell (1924)
Yak	× Dom. cow	3	8	Deakin *et al.* (1935)
Dom. bull	× Yak	1	0	Rothwell (1924)
Dom. bull	× Yak	1	1	Deakin *et al.* (1935)
Yak	× Bison	0	1	Rothwell (1924)
Yak	× Bison	0	1	Deakin *et al.* (1935)
Eland	× Afrikander	1	0	Warren (1932)
		20	29	(40.82 ± 4.73)
Zebu	× Gayal	0	3	Bartlett (1884)
Gayal	× Dom. cow	13	14	Nathusius (1911a)
Gayal	× Dom. cow	9	9	Kühn (1888)
		22	26	(45.8 ± 4.85 per cent males)
Zebu	× Dom. cow	12	9	Nathusius (1911b)
Dom. bull	× Zebu	4	2	Nathusius (1911b)
Zebu	× Dom. cow	4	2	Pucci (1914)
Bos taurus	× *B. indicus*	1244	1109	Col. Matson (*In litt.* 1931)
		1264	1122	(52.95 ± 0.69 per cent males)

Goodnight (1914) states, that he has never had any male hybrids born alive

from matings of the American bison bull with domestic cows. The cows carrying bull calves either aborted or the calf died at birth. He further points out that only a few females are obtained and when these were bred to bison bulls they produced fertile females and infertile males. But when mated to the domestic bull they gave fertile offspring of both sexes. Iwanow and Philiptschenko (1916) reported sterile males and fertile females, when hybrid females were mated to either bison or domestic bulls. These authors also report similar results when the European bison was mated to domestic cows.

The numbers in crosses involving the zebu (*Bos indicus*), yak, domestic cattle, also eland and Afrikander, are small, being only 20 males and 29 females. The sex ratio is 40.82 ± 4.73 per cent males, which is low but due to small numbers the difference may not be significant. Males from these crosses are reported as sterile, while the females appear to give reasonably high fertility. Zawadowsky (1931) reported successful matings for both of his female zebu-yak hybrids. Pucci (1914) mentioned a total of 113 matings (zebu and domestic cow) and it was pointed out that the matings gave 92 per cent fertility—but sexes were given for only 6 calves.

Hybrids reported from crosses of the gayal with zebu and domestic cattle total 22 males and 26 females. Thus the sex ratio is 45.8 ± 4.85 per cent males. Because of the small numbers on which the ratio is based the difference between this ratio and that normal for the genus is not considered significant. Again the males were usually found to be sterile. Nathusius (1911a), however, reported a gayal-domestic cow hybrid bull that produced 11 calves from 21 matings. The F_1 female hybrids recorded by Nathusius were continued in breeding until 140 calves were subsequently produced. The proportion of the sexes is essentially equal, there being 69 males and 71 females.

b. Sex ratio of *Bos taurus* × *B. indicus* hybrids

Through Colonel Matson unpublished records were obtained for the sexes of hybrids between *Bos taurus* males and *B. indicus* females. The parentage was known for 1244 males and 1109 females, which is 52.87 ± 0.69 per cent males. (Parentage was uncertain for an additional 165 males and 127 females.) Sexes previously recorded for this cross bring the totals to 1264 males and 1122 females. The sex ratio is, therefore, 52.95 ± 0.69 per cent males, which is slightly higher than the normal ratio reported for the parental species. Since the sex ratio for these hybrids is even higher than that for the parental species, the results are not in accord with the view that a deficiency of males is produced by hybridization or else this should not be considered a species cross. According to Morse (1910), Lydekker recognized this fact. He suggested that all of the forms without the hump should be designated *Bos taurus typicus* and all of those with the hump *Bos taurus indicus*. The crossing results certainly favor this interpretation of their relationship, and it is further supported by the fact that the progeny are apparently fully fertile with the parent species.

4. Sex ratio of miscellaneous hybrid mammals

A summary of the sexes reported for miscellaneous hybrid mammals is given in Table 4. Detlefsen (1914) obtained 14 males and 23 females in an F_1 from a wild Brazilian cavy male (*Cavia rufescens*) mated with domestic (*C. porcellus*) females. This is a ratio of 37.84 ± 5.38 per cent males. The normal sex ratio for the

TABLE 4

Sexes of miscellaneous hybrids

CROSS	M.	F.	SEX RATIO	AUTHORITY
Brazilian cavy × Dom. cavy	14	23	37.84 ± 5.38	Detlefsen (1914)
Mus bactrianus × *M. musculus*	104	101		Green (1930)
Mus musculus × *M. bactrianus*	29	31		Green (1930)
	133	132	50.19	
Goat × Ewe	0	5		Spillman (1913)
Goat × Ewe	13	4		Buffon (1791)
Goat × Ewe	1	4		Weir (1888)
Goat × Ewe	1	1		Kulaginn (1889)
	15	14	51.72	
Mouflon × Dom. Sheep	91	121	42.91 ± 2.29	Kühn (1888)
Kamerunschaf × Karakul	6	3		Heck, and Steinmetz (*In litt.* 1932)[1]
Karakul × Heidschnucke	0	1		Dr. Lutz Heck (*In litt.* 1932)[1]
	6	4		
Dromedar × Kamel	1	0		Dr. Lutz Heck (*In litt.* 1932)[1]
Wolf × Dog	4	4		Noack (1887)
Wolf × Dog	1	4		Anonymous (1883)
Wolf × Dog	1	1		Steinmetz (*In litt.* 1932)[1]
	6	9	40.0 ± 8.56	
European wolf × Dingo	0	6		Anonymous (1932)
Wild boar × Dom. sow	3	8		Spiller (1894)
Wild boar × Dom. sow	11	9		Culbertson (*In litt.* 1932)[2]
	14	17	45.16 ± 6.0	
Hare × Rabbit	0	1		Thursfield (1830)
Hare × Rabbit	0	1		Lönnberg (1905)
Hare × Rabbit	1	1		Kuiper (1925)
	1	3	25.0	
Mustela putorius × *Martes furo*	2	3		Pitt (1921)
Mustela erminea × *Mustela furo*	1	3		Cocks (1899)
Otaria pussila × *O. calif.*	1	0		Jennison (1914)
Ursus americanus × *U. arctos*	1	1		Bartlett (1860)

[1] Information furnished in letters from Dr. Lutz Heck and Dr. H. Steinmetz, Berlin Zoological Graden.
[2] Unpublished records Ia. Agr. Exp. Sta. through courtesy of Mr. C. C. Culbertson.

TABLE 4—*Concluded*

CROSS	M.	F.	SEX RATIO	AUTHORITY
Lemur macaco × *L. fulvus rufif.*	0	1		Pocock (1911)
Bubalus lunata × *B. caama*	1	0		Selous (1893)
Massai × Atbara-Pavian	2	0		Heck, and Steinmetz (*In litt.* 1932)[1]
Braungraue Meerkatze × Campel	1	0		Heck, and Steinmetz (*In litt.* 1932)[1]
Löwen × Tiger	1	0		Heck, (In litt. 1932)[1]
Barbary Ape × Mangaby Monkey	1	0[2]		
Mus rattus × *M. decumanus*	1	0		Van Kemper (1899)
Taurotragus oryx × *Strepsicros capensis*	1	0		Boulineau (1933)
Taurotragus oryx × *Condou*	0	1		Boulineau (1933)
Dog × Fox	1	0		Priesner (1933)
Pariah-dog × Jackal	2	1		Hurst (1933)
	16	10		

[2] Information supplied by Mr. Fred Winkelmann, Supt. Vilas Park Zoo, Madison, Wisconsin.

guinea pig as reported by Ibsen (1923) is 51.67, Wright (1922) 50.4, Schott and Lambert (1930) 49.45 per cent males. Again the F_1 males were sterile and the females were fertile. Back-crosses were made by breeding the hybrid females to *porcellus* males, from which 31 males and 52 females were produced. These females mated to *porcellus* males gave 101 males to 116 females, and in the next cross 159 males to 153 females were produced. Detlefsen stated, that with each subsequent back-cross increasing signs of fertility of the males appeared.

Sexes for F_1 hybrids between two species of mice have been reported by Green (1930). The males total 133 and the females 132. Both sexes were fertile in back-crosses. Green's data show an increase in the proportion of males obtained from the back-crosses over that of the F_1, but the numbers are too small for significance.

Parkes (1924) reports a sex ratio of 118.0 (54.1 per cent) in the domestic mouse, but notes that Schultze (1903), with about the same numbers found approximate equality. Karol (1928), summarizing the results obtained by Sumner, on three subspecies of deer mice (*Peromyscus polionotus*), reports a sex ratio of 103.01 ± 1.64 (50.7 per cent) for the species as a whole. The ratio for subspecific hybrids in this species was 114.61 ± 5.79. Subspecific hybrids mainly of *P. maniculatus* gave a lower ratio. In general, however, it may be said that the differences in the ratios of the several species and of these subspecific hybrids are of doubtful significance. Apparently in these cases subspecific differences are not sufficient to affect seriously the sex ratio.

The goat and sheep crosses shown in Table 4 show a sex ratio as near equality as could be expected for only 29 individuals. Much doubt is entertained regarding this cross, although Buffon specifically states that he made the cross for those which he reported. The matings for the others are not vouched for by those who gave the reports. Kühn (1888) reported that in several hundred matings of goats with sheep no offspring was produced. Warwick, Berry and Horlacher (1934) reported 48 matings of rams with Angora female goats, in carefully planned and conducted experiments and although 22 conceptions occurred none of the embryos survived to the end of a normal gestation period.

Kühn (1888) observed 91 males and 121 females in an F_1 from crosses of mouflon and domestic sheep. By mating the hybrids he secured an F_2 with 43 males to 47 females, and an F_2 with 16 males and 11 females. The sex ratio for the F_1 is 42.92 ± 2.29 per cent. Although the numbers are small the sex ratio approaches equality in the subsequent generations. Numerous authors, notably Nichols (1924), White and Roberts (1927), Chapman and Lush (1932), and Johansson (1932b), have reported the sex ratio for domestic sheep to be slightly below equality (about 49 per cent males). Frolich (1936) reported observations on a flock of Karakul sheep which show a sex ratio of 52.3 ± 5.9 per cent. The numbers are small, however, being only 304.

The ratio for the F_1 hybrids in Kühn's experiments is about 6 per cent below that which appears to be the normal but the difference is not statistically significant. This, together with the fertility of the hybrids, rather clearly indicates that the low sex ratio found for this cross may be due to chance. If Kühn's conclusion that mouflon and domestic sheep belong to the same species is correct, a disturbance of the sex ratio would not be expected to result from crossing the two.

Reports of the sexes of the F_1 for crosses of the wolf and the dog show a total of 6 males and 9 females. While females are in excess the numbers are too small to be significant.

Two groups of hybrids between wild and domestic swine are reported. The hybrids were reported to be fertile and for the small numbers the sex ratio is probably not disturbed. Therefore, this is not likely a wide cross.

Discussions in the literature show controversy of long standing regarding the hare and rabbit cross. Nachtsheim (1935) has presented a comprehensive review of the conflicting views of this controversy. He points out that the few controlled experiments, including his own, have yielded negative results. Kuiper (1925) discussed Houwink's experiments and reported 13 F_1 hybrids for this cross. He also mentioned that 34 offspring were obtained from matings involving the F_1. Sexes were mentioned for only one litter, which contained one individual of each sex.

The numbers for the miscellaneous hybrids shown at the bottom of Table 4 are too small to indicate sex ratios. These are included with the hope that some readers may be interested in the particular matings involved.

DISCUSSION

While the primary sex ratio is not known for any mammal, such evidence as there is on foetal sex ratios shows a higher proportion of males in the earlier stages. This would indicate a still higher primary ratio, if the trend holds as the curve, plotting the sex ratio, is projected back to the time of conception, with many more males than females conceived. As already stated, the secondary ratio approaches equality and this could result only from a compensatory high death rate of males.

1. *The foetal sex ratio*

Records of foetal sex ratios are meager but for the most part those which have been reported show a significantly higher proportion of males than occurs at birth. Jewell (1921) observed the sexes for 1000 foetal calves and found a ratio of 55.2 per cent males. Crew (1925) and Parkes (1925) concluded from a study of the sexes of pig embryos that the foetal sex ratio for this species is approximately 60 per cent males. Ibsen (1928) observed the proportion of the sexes late in the

gestation period for the guinea pig and found 55.95 per cent males. Crew (1927) gave a comprehensive discussion of the sex ratio in man and pointed out that the ratio for abortions and still-births is distinctly higher than the secondary ratio. He concluded that the primary sex ratio for man must be at least 170 males to 100 females (about 63 per cent males). Furthermore, it is pointed out by Crew that the proportion of males decreases continuously during foetal life and until about the fifth year after birth. Parkes (1926) gave a summary of the sex ratios of aborted foetuses for several species and concluded that the sex ratios are appreciably higher than those for live-born young. He also observed that in several polytocous species the number of young born was less than the number of corpora lutea. Hammond (1914) found an appreciable difference in the number of corpora lutea and the number of foetuses present in both the sow and the rabbit. Corner (1923) observed that about 20 per cent of the ova in the sow were missing before the end of the fourth week of gestation, and a further loss occurred in some sows during the latter course of pregnancy so that at term about 25 to 30 per cent of the originally discharged ova were either totally missing, or the foetuses were degenerating or abnormal. MacDowell and Lord (1925, 1926) reported observations on counts of the number of corpora lutea in pregnant mice and young sexed immediately after birth and it was found that, in 106 litters where the number of corpora lutea exactly corresponded to the number of young born, the secondary sex ratio was equality. These authors maintain that the evidence for the conclusion that the male foetus is less viable than the female is incomplete, and they question the belief in a mammalian primary sex ratio being above equality.

Düsing (1887), in his studies of the sex ratio for the horse, found the number that died *in utero* or within one month after birth gave a sex ratio of 157 males to 100 females. This is a ratio of 61.09 per cent males. Data presented in the present study for still-born horse colts show a sex ratio of 77.5 ± 4.4 per cent males. But these data are not complete because there were 7 pairs of still-born twins for which sex was not recorded. Little is known regarding the completeness of Düsing's data. Furthermore, his ratio does not represent that of still-births alone since deaths for one month after birth are included. However, these data indicate that foetal mortality may fall heavily on males in this species.

Records presented for mule foals show a slightly higher percentage of still-births than those recorded for the horse. But the difference between the sexes of still-born mules is insignificant.

These facts are interpreted as indicating that males are conceived more often than females. Also that the equality of the secondary sex ratio is probably due to a higher mortality of male than female embryos. A foetal sex ratio above equality in mammals does not necessarily conflict with the modern chromosome theory of sex determination, since there are several possible ways to account for more male than female zygotes being conceived.

It has been frequently suggested that there may be a difference in the ability of the two types of spermatozoa to make successful contact with the ova, and that this might be accomplished by a difference in the rate of movement of the two types of spermatozoa toward the egg. If the Y-bearing spermatozoon is smaller and thereby lighter than the X-bearing it might be favored in the journey toward the egg. Some evidence in support of this suggestion has been reported.

Wodsedalek (1913, 1914, 1920) observed a size dimorphism in the spermatozoa of the pig, horse and bull. Parkes (1923) reported a size difference for the spermatozoa of man, the rat and the mouse. But Krallinger (1928), and Williams and Savage (1925) did not find this in their studies for the bull. Moench and Holt (1929) also failed to find a size difference in the spermatozoa of the pig and man. Zeleny and Faust (1915) observed a slight difference in the spermatozoa of the ram, bull and dog. Lush (1925) observed a slight size difference in the spermatozoa of the rabbit and the pig. He also attempted to separate the spermatozoa into two size groups for the rabbit by centrifuging the seminal fluid. Sex ratios obtained by artificial insemination with the centrifuged spermatozoa of the two size group samples were apparently unaffected.

Evidence obtained from double matings tends to show that there may be a differential "potency" of spermatozoa from different males. Cole and Davis (1914) demonstrated this with rabbits by simultaneous matings of a pigmented and an albino male to albino females. In 23 matings 190 young were born of which 166 were by the pigmented male. For 15 of the 23 litters the spermatozoa from the pigmented male apparently succeeded in fertilizing all of the eggs. The difference was probably not due to actual low fertility of the albino male, for when used alone he gave good fertility. As pointed out by the authors this might be accounted for on the basis of a difference in the vitality of the spermatozoa, expressed in a greater or less motility or a difference in the penetrating power of the gametes of the two males. Similarly, such a difference might exist between the two different types of spermatozoa produced by a single male. Thus an excess of one sex might be expected.

King (1918) in discussing the difference between the sex ratios of two strains of rats produced by inbreeding and selection states that the results seem to show that in the rat the female has a greater influence than the male in determining the sex ratio. Furthermore, King suggested that the greater the difference of blood relationship between individuals the stronger is the attraction between the ova and the male-producing spermatozoa. Obviously a low sex ratio among interspecific hybrids does not harmonize with this suggestion.

Since the available evidence indicates a larger proportion of males than females among foetuses and the ratio at birth approaches equality, there must be a disturbance of some sort that is responsible for a higher death toll among male embryos.

A few hypotheses have been suggested. It has been mentioned frequently that sex-linked lethal genes may be operative in placing a greater mortality on the male sex, because of its possession of only one X-chromosome. MacArthur and Baillie (1932) object to this on the ground that the sex mortality relations expected in avian and lepidopterous forms on this theory is not found. Furthermore, they maintain that it is not clear that the theory even explains the mortality relations in *Drosophila* type forms. They state that, considering the relative number and size of the sex chromosomes, fewer sex-linked genes, and less sex-differential mortality, would be anticipated in man than in *Drosophila*, and in both less than in *Hymenoptera*, where haploid males may receive unfavorable recessive factors in each chromosome. But the possibility remains that sex-linked lethal genes may be a factor affecting foetal mortality of males in mammals.

Riddle (1927) points out that the serological studies at the Frauenklinik at

Halle indicate that the blood of the mother reacts to a male foetus as to a foreign body, and that an "antitestis" substance may enter the foetal blood and induce a reaction in the male foetus. He also states that the mother's blood does not appear to react similarly against a female foetus, and that the intrauterine environment is not equivalent for the male and the female embryo. Lillie (1917) offered the suggestion that a disturbance of the equilibrium that protects the male from the sex hormones of the mother during pregnancy may result in malformations of the male sex characters and that this might cause a greater mortality among male foetuses. A somewhat similar hypothesis was advanced by Kostoff (1931) to account for unsuccessful interspecific matings. It was suggested that antibodies are produced by the maternal organism, against hybrid embryos, which may result in sterile matings. The hypothesis was confirmed by immunological experiments with plants but the results were less positive in tests with animals.

MacArthur and Baillie (1932) gave a comprehensive discussion of the differences in the death rates of the sexes for numerous species, and presented evidence which indicates that as a rule the death toll is greater on the male than on the female sex in all groups of animals, irrespective of their homogamety or heterogamety. Numerous researches have shown that for several different species the male has a higher metabolic level than the female. Riddle pointed out that this would involve increased nutritional and oxygen demands and might therefore result in a higher prenatal death-rate on the male side.

Gowen (1931a, 1931b), working with *Drosophila*, observed that individuals possessing unbalanced groups of chromosomes had a shorter life span than those that had balanced groups. Furthermore, he reported that qualitatively the larger the production of CO_2 per day the shorter the time the flies were capable of living. Flies with balanced chromosome groups produced less CO_2 than those with unbalanced groups.

Should maleness itself impose a higher metabolic level than femaleness and chromosome unbalance tend to accentuate the rate of metabolism, it appears possible that the rate of metabolism might become a more deleterious force among males in mammals, since that sex may have a less favorable chromosome balance due to its having only one X-chromosome. If antibodies or hormones produced by the mother were the only factors affecting foetal mortality, a deficiency of males might be expected among birds, since eggs are known to contain maternal hormones. Although the sex ratio among birds has been reported slightly below equality some reports show an excess of males, and among hybrid birds it is the female sex that shows a deficiency in numbers. Therefore, the view that female hormones or antibodies are responsible for a greater embryonic death toll of males than females does not harmonize with the facts for both mammals and birds.

In view of the evidence which various researches have yielded on the question of prenatal mortality and sex ratios it appears improbable that one factor alone can be relied on to account for a higher death toll among male than female embryos in all cases. Apparently, at least three factors may be involved. First, sex-linked lethal genes may disturb the normal proportion of the sexes in some instances; second, since the Y-chromosome apparently does not as a rule carry genes the male (or heterogametic) sex may receive fewer dominant genes than the female (or homogametic sex) in some cases,

and in such cases the male may actually lack the vigor possessed by the female; third, evidently maleness imposes a higher rate of metabolism than femaleness, and in heterogametic males this may become a deleterious force, since the possession of only one X-chromosome might result in a less favorable gene balance which may accentuate the metabolic rate.

All of these factors may be operative to a varying degree in interspecific and intergeneric crosses. In certain crosses there may be a pronounced degree of unfavorable action due to these forces and it is possible that a markedly disturbed sex ratio would result in such instances. In other crosses only one or two of these factors may be involved, to a lesser extent, and in such cases the disturbance may be imperceptible or slight. Since in general in crosses of widely separated species there is a greater disparity between the numbers of the sexes than for more closely related species it appears that chromosome unbalance may be the disturbing factor of most importance.

Furthermore, chromosome unbalance may be so great in some crosses that even the homogametic sex can not survive. In such cases the matings would be completely sterile.

2. *Sterility of hybrids*

Sterility among hybrid mammals appears to be much more frequent in males than females. It has been pointed out by numerous observers that sterility results from chromosome incompatibility in hybrids in the reduction phases of gametogenesis. The degree of disturbance may be less in the female hybrid because all members of the pairs of chromosomes have similar mates in this sex. Furthermore, irregular chromosome behavior in meiosis may occur more often in the heterogametic sex because of a greater dissimilarity of the X- and Y-chromosomes; but still the exact cause is not clear. As pointed out by Huskins (1929) and observed by Clausen (1930) abnormal chromosome behavior may occur within a pure species where the members of each pair, as far as is known, are homologous. But the possibility of chromosome or gene balance as a factor effecting fertility in hybrids appears to remain.

3. *Criteria of hybridity*

Data secured in this study emphasize the need for a more reliable criterion of hybridity than we now have. Sterility of the hybrids was once thought to be a very definite rule, but this does not fully suffice since sterility may occur in only one sex in some cases and in both in others. Furthermore, sterility may occur within a breed, or within a strain of a breed. However, in general sterility may indicate hybrid relations and it is therefore still considered one of the indices of hybridity. Irregularities in meiosis may be characteristic of hybridity as has been pointed out particularly by Jeffrey (1928), Wodsedalek (1916), Zuĭtin (1930), Renner (1924) and Federley (1923). However, Huskins (1929), objects to irregularities in meiosis as a criterion because some interspecific hybrids may show regular maturation divisions and still be sterile; and because irregularity of meiosis can be induced by agencies other than hybridization. Serologic tests were invoked by Landsteiner and van der Scheer (1924), and Walsh (1924). The more recent work of Irwin, Cole and Gordon (1936), Irwin and Cole (1936a) and (1936b) on immunogenetic relations in birds demonstrates further the applicability of this test as a criterion of hybridity.

4. *Fertility of the mule*

Cases of fertile female mules have been recorded by numerous observers notably Hensler (1925), Jones (1916), (Jones also

reported a fertile female hinny), Braashuus-Jessen (1930), Haworth (1928), Goldsmith (1917), Ewart (1910b), Eichorn (1929), Warren (1926) and Groth (1928), Warren (1933), and Gramlich (in *litt.* to author, 1936). In some of these cases the observers expressed doubt as to the mother truly being a mule, or as to the young actually being from the animal claimed to be the mother. But the case reported by Groth was subjected to the serologic test, and the blood of the mother gave the reaction characteristic of mules. Furthermore, this mule produced young while in the possession of the Texas A. & M. College, where critical attention was given to mating and foaling. It appears that this case should establish the fact that female mules may in some cases be fertile.

So far as known to the writer no case of fertility of the male mule has been recorded. Histological studies of the testis of the male mule have been reported by Stephan (1902), Whitehead (1908), Wodsedalek (1916), and Goldsmith (1917). Various disturbances which indicated complete sterility were found. Ewart (1910b) observed the seminal fluid from one of his zebra-horse hybrids and found tailless or almost tailless spermatozoa which were immobile.

5. *General remarks*

Crosses involving bison and domestic cattle result in a markedly disturbed sex ratio, and fertility of the female hybrids is low, while the males are completely sterile. Therefore, the facts appear to favor the classification of these two forms in a separate genus as is now the case. Although the yak is placed by some zoologists in the genus *Poephagus*, the fact that the sex ratio of hybrids from crosses of the yak with species of the genus *Bos* is not seriously disturbed and female hybrids appear to give good fertility, and that males are occasionally fertile, tends to favor the classification of this form in the genus *Bos*. Apparently American zoologists generally agree that it belongs in this genus.

The sex ratio and the fertility of the hybrids from crosses of Zebu and domestic cattle is in perfect harmony with the modern view that these animals belong to the same species.

The two species of mice (*Mus bactrianus* and *M. musculus*) crossed by Green (1930) gave a sex ratio essentially equal to that normal for the parent species. The hybrids were also fertile. Therefore, the question of a true specific difference between these two forms is raised.

When all the known criteria of hybridity are applied to the mule, evidence that the horse and the ass belong to different species is convincing. However, the two species are known to cross with little difficulty and the issue of the cross (the mule) has proved to be of unusual value. As a result of the crossability of the two species and the utility of the hybrid, mules have been produced since early times. The cross is commonly made only one way in the United States; namely, by mating jacks to mares. It is rather generally claimed that the issue of the reciprocal cross (the hinny) is more horselike than the mule, and lacks the hardiness characteristic of the mule. But this is doubted by some observers. Definite proof for either view seems to be lacking.

SUMMARY

1. Individual hybrids between different species and genera of mammals, for which sex is recorded by various observers, are summarized. On the whole the sex ratio is appreciably below equality, i.e. the females are in excess.

2. Original records on the sexes of 1416 mule foals sired by 98 different jacks show a sex ratio of 44.28 ± 0.89 per cent males.

3. Foaling records were obtained for 1263 horse foals. The sex ratio for these is 52.52 ± 0.95 per cent males.

4. The frequency of twin births for mules was 0.71 per cent. For the horse foals the frequency of twinning was 1.12 per cent.

5. A high percentage of males was found among the still-born horse twins, 77.5 per cent. Also 64.3 per cent of the twin horse foals were still-born. But only 4.28 per cent of all of the horse foals were still-born.

6. Still-born mule foals totaled 5.01 per cent of the total births. The numbers of each sex still-born are essentially equal.

7. The sex ratio found for the mule is 8.24 per cent below that for the horse in the data presented. This difference is statistically significant.

8. Records of the sexes of 2353 individual hybrids, of which the parentage was known, between *Bos indicus*, and Bos taurus were secured from Colonel Matson of India. The sex ratio for these is 52.8 ± 0.69 per cent males.

9. Records for 8770 calves of *Bos indicus* were also obtained from Colonel Matson, and the Louisiana Agricultural Experiment Station. The sex ratio of these is 50.92 ± 0.36 per cent males. This is slightly lower than the ratio reported for *Bos taurus*, which is 51.52 per cent males.

10. Data presented in this study are in agreement with the view that if a disturbance of the sex ratio results from hybridization the deficiency is found to be in the heterogametic sex.

11. Reports on the foetal sex ratio are cited and the evidence is interpreted as indicating that in mammals males are probably conceived more often than females. Furthermore, the evidence appears to indicate that the foetal death toll generally falls heavier on the male than on the female sex.

12. It is suggested that the differential mortality of the sexes is due to the action of at least three factors: (1) sex-linked lethal genes in some instances; (2) since the Y-chromosome apparently does not in general carry genes the male may receive fewer dominant genes than the female and therefore may be actually weaker in some cases; (3) evidently maleness imposes a higher rate of metabolism than femaleness, and in heterogametic males this may become a deleterious force, since the possession of only one X-chromosome might result in a less favorable gene balance, which may accentuate the metabolic rate.

13. It is assumed that all of these factors may be operative in varying degrees in interspecific and intergeneric hybrids, and that chromosome unbalance may result in a rise of the rate of metabolism above the level of adaptation, among hybrid embryos, but more frequently in the heterogametic sex because of the presence of only one X-chromosome. Thus a greater mortality of the heterogametic sex would result.

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THE METABOLISM OF PATHOGENIC TRYPANOSOMES AND THE CARBOHYDRATE METABOLISM OF THEIR HOSTS

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INTRODUCTION

TRYPANOSOMES are parasitic protozoa that live in the blood of a great variety of species in all classes of vertebrates and that are generally transmitted by an invertebrate intermediary host. A characteristic morphological feature of the forms that live in the blood is an undulating membrane along which runs a flagellum. This terminates at the posterior end in the blepharoplast, the kinetic center of the cell, and at the other end in a free flagellum. In many cases, different species are so much alike that they cannot be distinguished morphologically, but only by means of biological experiments. In other cases, they can readily be recognized by morphological peculiarities. A few types, which may illustrate this, are shown in Fig. 1. Most of the species, for example *Trypanosoma rotatorium*, the first described trypanosome (Gluge, 1842), and all the others occurring in fishes, amphibians, reptilians and birds are without injurious effects on their hosts. The same is true for most trypanosomes which live in mammals. But the group is nevertheless of great interest both from a public health and an economic standpoint, since some very pathogenic trypanosomes are found in man and domesticated animals. Human sleeping sickness in Africa is caused by *Trypanosoma gambiense* and *T. rhodesiense*, the former being the first trypanosome found in man, and first described by Dutton. In South America *Trypanosoma cruzi* is, according to Chagas, responsible for a human disease. *Trypanosoma brucei*, *T. evansi*, *T. equiperdum*, *T. equinum* and some others cause severe diseases of domesticated animals in various parts of the world. It is therefore not astonishing that many investigations have been carried out on the various aspects of the biology of these pathogenic forms. In the present paper the metabolism of the pathogenic trypanosomes and the problem of the relationships of this metabolism to the injuries suffered by the host during the infection are reviewed.

SCHERN'S "REVIVING-PHENOMENON"

Schern (1912) showed that it was possible to revive trypanosomes *in vitro*, which had lost their motility, through the addition to the medium of serum or liver-extract of normal animals, but not of animals suffering from heavy trypanosome infections. Because this observation suggested possible implications concerning the mechanism of pathogenicity of the trypanosomes it gave during the last decade the impulse for many investigations concerning both the metabolism of the host and of the parasites.

Schern's reviving-phenomenon was apparently due mainly to the presence of glucose in the serum and liver-extract. It was possible to get a similar effect with glucose, levulose, mannose or glycerol, whereas galactose and maltose were less favorable and many other substances had no influence on the viability of the parasites (Schern, 1912, 1925, 1928; Kudicke and Evers, 1924; Dubois, 1926, 1930; von Jancsó and von Jancsó, 1935). These findings, combined with the demonstration that both *Trypanosoma lewisi* and pathogenic trypanosomes survived longer *in vitro* in the presence of carbohydrates (Biot, Biot and Richard, 1911; Fleig, 1911; Poindexter, 1935) and that glucose is essential in culture media used in the maintenance of many trypanosomes (first recognized by Hagemeister, 1914), suggested that the sugar is used by the parasites. The experiments mentioned, however, were not definitive proof that this assumption is justified, since von Fenyvessy (1926) could obtain Schern's reviving-phenomenon with sugar-free solutions,—for example, sugar-free broth—and since Dubois and Bouckaert (1927) and Bruynoghe, Dubois and Bouckaert (1927) tried without success to demonstrate the actual sugar consumption *in vitro*.

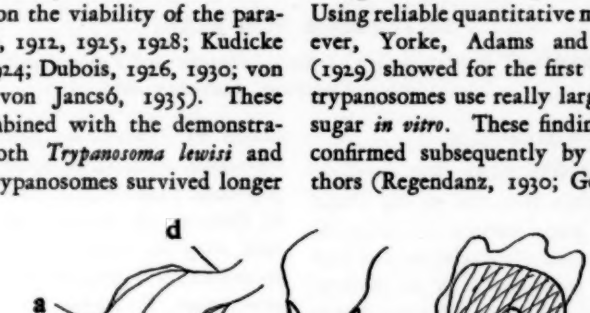

FIG. 1. VARIOUS TYPES OF TRYPANOSOMES

(1) *Trypanosoma lewisi*, nucleus in the anterior part of the body. (2) *T. gambiense*, nucleus often in the posterior part of the body. (3) *T. cruzi*, characterized by the big blepharoplast. (4) *T. equinum*, characterized by having no blepharoplast. (5) *T. rotatorium*, broad form with striped pellicula. (*a*, nucleus, *b*, undulating membrane, *c*, free flagellum, *d*, blepharoplast.)

METABOLISM OF TRYPANOSOMES

Sugar metabolism of the trypanosomes. Using reliable quantitative methods, however, Yorke, Adams and Murgatroyd (1929) showed for the first time that the trypanosomes use really large amounts of sugar *in vitro*. These findings have been confirmed subsequently by different authors (Regendanz, 1930; Geiger, Kligler and Comaroff, 1930; von Fenyvessy and Scheff, 1930; von Issekutz, 1933; von Brand, 1933; Reiner and Smythe, 1934; von Jancsó and von Jancsó, 1935; Reiner, Smythe and Pedlow, 1936). It has been shown that the pathogenic trypanosomes, with the exception of *Trypanosoma cruzi*, consumed quantities of sugar amounting to about 7–8 mg. per 1000 millions animals per hour at 37°C., whereas the non-pathogenic *Trypanosoma lewisi* consumed under similar conditions only about 1.5 mg. of sugar. The intensity of carbohydrate metabolism is therefore extremely

high. This is strikingly evident if we consider the fact that as 1000 millions of trypanosomes correspond to approximately 60 mg. of fresh substance only, they consume during 24 hours about twice their own weight of sugar, whereas the energy requirement of a 70 kg. man for the same period is contained in about 500 gm. of sugar. This difference is probably due to three facts: (1) The trypanosomes are extremely active organisms. (2) They are very small and their relative surface is much greater than that of man, and (3) they utilize only a fraction of the energy contained in the carbohydrate, since they do not totally oxidize the sugar to carbon dioxide and water.

The rate of sugar consumption is, as in other animals, in a high degree dependent upon the temperature (Regendanz, 1930), the temperature curve resembling Krogh's normal curve more than a van't Hoff curve (von Brand, 1933). Opinions are divided as to whether the trypanosomes are able to maintain the same level of carbohydrate consumption in media of different sugar concentration. Regendanz (1930) and von Jancsó and von Jancsó (1935) found the carbohydrate metabolism less pronounced in lower sugar concentrations than in higher ones and the same was reported for very low concentrations by von Fenyvessy and Scheff (1930); von Brand (1933), on the other hand, observed a uniform rate of sugar consumption at least in a range of concentrations which may occur in the blood of an infected animal. The same difference of opinions is based upon experiments *in vivo*. Krijgsman (1933) could obtain no changes in the course of the infections by injecting his experimental animals with insulin, indicating that enough sugar was always present in the blood for the parasites. Poindexter (1935), on the other hand, states that in animals subjected to insulin treatment the infection lasted longer than in the controls and fewer parasites were present. He assumes that this was due to the lowering of the blood-sugar level, which would make less favorable conditions for the development of the trypanosomes.

The quantitative experiments mentioned above were carried out with glucose, but this is not the only sugar which can be used. Of the pathogenic trypanosomes only *Trypanosoma brucei* has been examined quantitatively in relation to its consumption of other carbohydrates. It was found (von Brand, 1933) that this organism could satisfy its sugar requirements only with glucose, mannose, maltose, levulose and galactose. The corresponding relative consumption rates were 100:86:50:21:9.

Apparently trypanosomes in general do not accumulate carbohydrate reserves in their body in the form of glycogen (Krijgsman, 1936), though Schern and Bozzolo (1930) seem to have found a small amount of glycogen. This question is not of great importance, since even relatively large reserves would last only a very short period because of the enormous intensity of the carbohydrate metabolism of the parasites. It seems that the blood-inhabiting forms of the pathogenic trypanosomes are absolutely dependent upon the presence of an available sugar in their surroundings. It is probable that sugar is their only source of energy (v. Brand, 1933, Krijgsman, 1936, von Jancsó and von Jancsó, 1935). Whether this is true also for their developmental stages is not yet sufficiently investigated (von Brand, 1935).

Oxygen consumption and end-products of sugar metabolism. As might be expected from blood-inhabiting organisms, the trypanosomes use oxygen as first shown by Nauss and Yorke (1911) and confirmed by several authors (von Fenyvessy and Reiner,

1924; von Fenyvessy and Scheff, 1930; von Issekutz, 1933; Reiner and Smythe, 1934; Reiner, Smythe and Pedlow, 1936). It was remarkable, however, that the amount of oxygen consumed was too small to account for the total oxidation of the sugar. The necessary consequence is that the end-products resulting from the carbohydrate metabolism, are only partially oxidized substances. It was first observed that during experiments *in vitro* carbon dioxide was liberated from bicarbonate and that the pH of the medium was lowered (von Fenyvessy, 1926; von Fenyvessy and Reiner, 1928; Kligler and Geiger, 1928; Kligler, Geiger and Comaroff, 1929; Geiger, Kligler and Comaroff, 1930) thus indicating the production of a relatively strong acid. Since the lactic acid content of the blood of animals infected with trypanosomes was found to be higher than normal and since the chief end-product of anaerobic carbohydrate metabolism in many animals is lactic acid, it was assumed that the same acid was also produced in this case. Subsequent investigations, however, (von Brand, Regendanz and Weise, 1932; Reiner and Smythe, 1934; Reiner, Smythe and Pedlow, 1936) have shown, that the parasites in question give off no lactic acid, or at least only very small amounts. It was apparent that one or more other acids were formed, indicating that the type of carbohydrate metabolism is rather related to that of parasitic worms than to vertebrates (von Brand 1934, 1935). Reiner and Smythe (1934) and Reiner, Smythe and Pedlow (1936) could follow the whole course of glucose breakdown in the case of *Trypanosoma equiperdum*. The first step under aerobic conditions and the only one realized under anaerobic conditions was the transformation of one molecule of glucose to one molecule of glycerol and one molecule of pyruvic acid. The next step was the oxidation of glycerol to pyruvic acid and water. The whole process is represented by the following formulae:

(1) $C_6H_{12}O_6 \rightarrow CH_3\,CO\,COOH + C_3H_8O_3$
(2) $C_3H_8O_3 + O_2 \rightarrow CH_3CO\,COOH + 2\,H_2O$

It follows that under aerobic conditions one molecule of glucose was decomposed to two molecules of pyruvic acid. It is obvious that this scheme leaves no place for carbon dioxide and lactic acid. As a matter of fact traces of both substances were found, but they may have originated from the metabolic activities of leucocytes, the presence of which is difficult to avoid completely in experiments of this type. It is of great interest that according to Reiner, Smythe and Pedlow (1936) the end-products of sugar metabolism are very different in the case of the non-pathogenic *Trypanosoma lewisi*. They identified as end-products under aerobic conditions succinic, acetic and formic acid, ethyl alcohol and carbon dioxide.

Fat- and protein-metabolism of the trypanosomes. There is no evidence that the pathogenic trypanosomes can use fats, since no fat-splitting ferments could be found (Califano and Gritti, 1930; Krijgsman 1936). Protein metabolism also seems to be relatively unimportant (von Brand, 1933; Krijgsman, 1936) and is probably, at least for the greatest part, confined only to the transformation of the protein substances taken from the blood of the host into the parasite protoplasm during the growth period. It is of interest that of ferments acting on proteins only cathepsine, carboxylpeptidase, amino polypeptidase and dipeptidase could be found, but no pepsine or trypsine, which are so widely distributed in other animals (Krijgsman, 1936). This is connected with the fact that the trypanosomes live in an environment in which enough pro-

tein degradation products are present to fulfill their nitrogen requirements.

CHANGES IN SUGAR METABOLISM OF HOST

Behavior of blood sugar during infection. As we shall see later on, it has been attempted several times to correlate the sugar metabolism of the trypanosomes with changes of the carbohydrate metabolism of the host and to explain in this way the lethal effect of the pathogenic trypanosomes on their hosts. In order to establish or test these theories many investigations have been carried out on the carbohydrate metabolism of parasitized vertebrates. Before we go into the details of these theories, we shall consider first some actual results of these experiments.

The first thoroughly investigated item was the behavior of the blood-sugar throughout the infection. Different animals were used as hosts: rat, guinea-pig, rabbit, dog, cat, sheep, horse and monkey. From man, also, a few data are known. The results of these observations are not completely uniform. In a few cases (Cordier, 1927; Savino, 1927; Tubangui and Yutuc, 1931; Walravens, 1931; Wormall, 1932) no noticeable changes were found, in others (Schern, 1928; Schern and Rossi-Lema, 1928; Andrews and Sanders, 1928; Angolotti and Carda, 1929; Scheff, 1932) a hyperglycemia was described for the whole course or at least for a part of the infection. In most cases, however, it was noticed that the blood-sugar level was lower than normal. This hypoglycemia occurred generally only in the last days or hours of a lethal infection (Schern, 1925, 1928; Regendanz and Tropp, 1927; Cordier, 1927; Dubois and Bouckaert, 1927; Bruynoghe, Dubois and Bouckaert, 1927; Knowles and Das Gupta, 1927/28; Zotta and Radacovici, 1929; Scheff, 1928, 1932; Linton, 1930; Locatelli, 1930; von Brand and Regendanz, 1931; Tubangui and Yutuc, 1931; von Brand, Regendanz and Weise, 1932; Krijgsman, 1933; von Jancsó and von Jancsó, 1935), but in a few cases the lowering of the blood-sugar was observed during longer periods or even throughout the greater part of the infection (von Fenyvessy, 1926; Dubois and Bouckaert, 1927; Cordier, 1927; Scheff, 1932; Poindexter, 1935). The totality of these experiments certainly indicates a disequilibrium in the regulation of the blood-sugar of an infected animal, most pronounced in the later stages of the infection and primarily characterized by the premortal hypoglycemia. The disturbance in the blood-sugar regulation is more significant in those cases, in which distinctly lowered values are observed at least some days before the death, whereas a hypoglycemia occurring only a few hours before the animal dies, seems to be of little importance only. Concerning this latter point it must be borne in mind that similar observations have been made in bacterial infections (Zotta and Radacovici, 1929) and in infections with *Bartonella muris* (Linton, 1929; Regendanz, 1929; Hoffenreich, 1932; von Brand, Regendanz and Weise, 1932) thus indicating that it is not specific for trypanosome infections.

Polysaccharide reserves of the host. This was soon recognized and attempts were made to go further into the carbohydrate metabolism of the host by studying the behavior of the polysaccharide reserves. Schern and Verokay (1925) and Schern and Bozzolo (1930), using morphological methods, recognized that the glycogen content was greatly reduced, or that the reserve polysaccharide was even absent in various organs of animals which had died of trypanosomiasis. Other authors tested the glycogen content of the host with quantitative methods and came essentially to the same conclusion. At least in the

late stages of the infection the glycogen content of the liver was almost invariably much lower and that of the muscles in most cases lower than normal. In a few cases it was even absent (Regendanz and Tropp, 1927; Bruynoghe, Dubois and Bouckaert, 1927; Linton, 1930; von Brand and Regendanz, 1931; Scheff, 1932; Krijgsman, 1933). Only in one dying monkey the liver glycogen was still high (Regendanz, 1929) and according to Bruynoghe, Dubois and Bouckaert (1927) even more glycogen was found in the muscles of infected than in uninfected mice and rabbits. These observations suggest again that the carbohydrate metabolism of the host is not normal. We shall find further proofs in considering the theories concerning the genesis of this disturbance to which we shall now proceed.

THEORIES CONCERNING THE LETHAL MECHANISM OF TRYPANOSOMES

Sugar consumption theory. Schern (1925, 1927) was the first who tried to explain the pathogenic action of the trypanosomes chiefly by their carbohydrate metabolism. The sugar consumption of the parasites is in his opinion so high that they rapidly use up the sugar present in the blood of the host. This process is so intensive that it is difficult for the liver to mobilize enough sugar from its glycogen deposits to maintain the blood-sugar level and therewith the carbohydrate metabolism of the body normal. The consequence of this hypernormal function is a great strain on the liver which finally can fulfill its other duties (detoxicating action, etc.) no longer. Eventually the liver-function breaks down and causes a glycopryvic intoxication of the body, from which death ultimately results. According to this theory (see also Schern and Artagaveytia-Allende, 1936) the sugar consumption of the parasites would be directly responsible for the death of the host. The fact that death occurs sometimes when there are still noticeable amounts of glycogen present in the liver is explained by Schern (1930) by assuming that in these cases glycogen can be mobilized no more (see also Andrews, Johnson and Dormal, 1930).

This last point has, however, never been proved experimentally, but there are, on the other hand, clear indications that the liver-function is disturbed. Schern and Citron (1913) found that infected rats excreted levulose in the urine after consuming such small amounts of this sugar only that in comparably treated normal animals no glycosuria occurred. Scheff (1932) found that under normal conditions the feeding of sorbite caused no changes of the blood-sugar level, whereas in infected animals a typical blood-sugar curve was found. He states further that the blood-sugar curve resulting from oral administration of sugar had a different form in infected than in normal animals. In view of the great variabilities of the blood-sugar curves in normal animals von Brand and Regendanz (1931) think that further evidence is necessary before this point can be settled definitively. These authors showed that the ability of the liver to synthesize glycogen from sugar was greatly diminished in infected animals. In some cases no glycogen formation at all could be found, even after great sugar doses. These deficiencies in the liver-function have a morphological basis in pathological changes of the liver tissue, which could be observed in several cases (Battaglia, 1926; Andrews, Johnson and Dormal, 1930; Hoeppli and Regendanz, 1930; Scheff, 1928). These points can doubtless be used in favor of Schern's opinions and his theory has been accepted by some authors (von Fenyvessy, 1926; Scheff, 1928, 1932; Knowles and Das Gupta, 1927/28), but the majority of the investigators doubt

that the sugar consumption of the parasites is directly responsible for the pathogenicity of the trypanosomes.

Asphyxiation theory. Andrews and Johnson (1929/30) and Andrews, Johnson and Dormal (1930) concede that the carbohydrate metabolism of the parasites might be responsible for the lowering of the blood-sugar level, but they believe that the liver lesions and the death of the host are rather due to asphyxiation. This is caused by pulmonary edema brought about by partial obstruction of the circulation by the agglutination of parasites in the blood vessels of heart and lungs.

Lactic acid theory. The idea that asphyxiation might be the ultimate cause of death had already been expressed by Kligler, Geiger and Comaroff (1929), but they assumed a very different mechanism. In their opinion great amounts of lactic acid are formed in the blood by the parasites during their sugar metabolism. This acid would effectively interfere, owing to a specific influence on the haemoglobin, with the oxydative processes of the body and the result would eventually be asphyxiation. There is no doubt that at least in the last stages of a trypanosome infection the lactic acid content of the blood is generally markedly increased. This has been shown repeatedly both with direct and indirect methods (lactic acid determinations, lowering of the pH or alkaline reserve of the blood) by Kligler and Geiger, 1928; Scheff, 1928; Kligler, Geiger and Comaroff, 1929; Dominici, 1930; Linton, 1930; Andrews, Johnson and Dormal, 1930; von Brand, Regendanz and Weise, 1932; Krijgsman, 1933. As stated above, however, the trypanosomes produce no lactic acid. Its accumulation in the blood is therefore obviously due to the metabolic disequilibrium of the host and is by no means linked directly with the sugar metabolism of the parasites. Krijgsman (1936) points out that the concentration of the lactic acid is too small as to injure the host.

Toxin theory. The last group of investigators (Regendanz and Tropp, 1927; Zotta and Radacovici, 1929; Locatelli, 1930; von Brand and Regendanz, 1931; Krijgsman, 1933, 1936) denies the possibility that the changes in the metabolism of the host and its death can be explained by the sugar consumption of the parasites or by the action of the hitherto defined end-products of their carbohydrate metabolism. These authors believe that it is necessary to assume the production of other as yet uncharacterized end-products of metabolism, which may be called toxins. They use the following arguments. The observed hypoglycemia cannot be explained by the sugar consumption of the parasites, since it occured also in animals; for example, rabbits which generally harbor so few parasites that their sugar consumption cannot play any great role. It was impossible to prevent the hypoglycemia of the host even by administration of large amounts of sugar. It has been shown, furthermore, that an infected animal had in most cases enough carbohydrate in its body to raise the blood-sugar level under experimental conditions. With the exception of Scheff (1932) all investigators (Regendanz and Tropp, 1927; Regendanz, 1929; Krijgsman, 1933) could observe an elevation of the blood-sugar level in hypoglycemic animals after injection of adrenalin. This view seems to be substantiated by the experiments of Scheff (1932) and Bruynoghe, Dubois and Bouckaert (1927), who observed a rapid rise of the quantity of blood-sugar to the norm or even above after killing of the trypanosomes by means of medicaments, even in starving animals (Scheff 1932). Infected animals also die, when they are fed with sugar (Cordier, 1927; Bruynoghe,

Dubois and Bouckaert, 1927; Andrews, Johnson and Dormal, 1930). Sometimes they lived a little longer (Dubois, 1928; Kligler, Geiger and Comaroff, 1930; Angolotti and Carda, 1929), in other cases the parasites seemed to have been stimulated by the sugar and the death of the host occured even earlier than in controls (Poindexter, 1933). Finally von Brand (1933) pointed out that the intensity of the carbohydrate metabolism of the parasites, though surprisingly high, is not high enough to allow the assumption of an effective interference with the metabolism of the host. He calculated that in a human being suffering from sleeping sickness the parasites consume at the most 2–3 per cent of the calories which are required for a resting man. In a rat the figure is, for the last days of an infection about 30 per cent, but a rat can doubtless starve several days without dying. It must further be borne in mind that experimental animals infected with *Trypanosoma cruzi* die, though according to von Brand (1933) this species consumed probably even less sugar than the non-pathogenic *Trypanosoma lewisi*.

The above discussion shows that the arguments which have been brought forward in favor of the toxin theory are based so far on negative indications. It must be admitted that the various attempts to actually demonstrate the existence of toxins in trypanosomes have failed. But it must be pointed out that these experiments have been designed to show the presence of poisonous substances in the body of the parasites (endotoxins). Whereas it is possible that the active substance is excreted by the parasites (Krijgsman, 1933). In his last papers Krijgsman (1936) states that the toxin might possibly belong to the amines which are known to be very poisonous and the formation of which he has shown to be likely by finding corresponding ferments. If this assumption could be proved, it would follow that the disturbance of the carbohydrate metabolism of the host has no connection with the sugar metabolism of the parasites, but with their protein metabolism.

SUMMARY

As a summary of the main results of the experiments reviewed above, the following points may be emphasized:

1. The carbohydrate metabolism of the pathogenic trypanosomes is characterized by its great intensity and also by the fact that the sugar is only partially oxidized in the presence of oxygen.

2. The carbohydrate metabolism of a mammal infected with pathogenic trypanosomes is disturbed, as shown mainly by the disequilibrium of the blood-sugar, lowering of the glycogen reserves and reduced ability to build glycogen from sugar.

3. Four theories have been presented to explain the disturbance of the carbohydrate metabolism of the host and its eventual death, three of which involve metabolic interrelationships of parasites and host. According to one the loss of carbohydrate suffered by the body from the sugar consumption of the parasites is responsible for the injuries of the host, another claims that a supposed end-product of the sugar metabolism of the trypanosomes (lactic acid) is the causative factor. The third theory assumes that toxins, perhaps originating from the protein metabolism of the parasites, play the decisive role in the pathogenicity of these organisms.

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THE GENETICS OF CANCER IN MICE

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IT HAS been known for many years that an important aspect in the susceptibility to different forms of cancer is that which deals with hereditary factors. The nature of this inheritance is still unsettled in many respects.

The purpose of this article is to tabulate the results of the various investigations and to place before the reader the different theories on the inheritance of cancer.

Since most of the results have been explained in terms of genetics, the principles of that science should be applied to any experiment, not only in regard to the material used but also to the interpretation given. Perhaps the most important of these principles to be considered in cancer experimentation work is that of the homogeneity of the stocks. In working with animals it is usually considered that a stock has attained a high degree of homozygosity after fourteen generations of brother-to-sister matings. This method of mating is employed by most geneticists.

The difficulty of obtaining homogeneity is emphasized in the following quotation taken from East and Jones (1919):

> Although nearly complete homozygosity is theoretically brought about by seven generations of self-fertilization, the attainment of absolute homozygosity is a difficult matter and in practice it may never be reached. . . . Continued selective mating is necessary to bring about homozygosity. Intermittent inbreeding alternating with periods of outcrossing, which is the prevailing state of affairs with many organisms, cannot maintain any high degree of homozygosity.

Several observations may be cited to point out the consistency of the cancer incidence obtained in several inbred strains of mice. Strong (1934) found that selection within the Strong "A" strain had no influence on the age incidence of breast cancer. Selection was started after the 28th generation of inbreeding. In regard to another stock known as the C_3H race Strong (1935b) also stated that, provided the individuals belonged to the same inbred strain, the difference between an animal which developed cancer and a non-cancerous individual was probably due to chance alone or to some intercurrent disease.

Breeding tests made by MacDowell and Richter (1935) showed that animals of the strain C58 with doubtful diagnoses of leukemia had the same genetic constitution as animals with positive diagnoses. Their results are given in the following table.

MacDowell and Richter found that the proportion of positive diagnoses among the offspring of leukemic parents was 88.8 per cent. In two other classes, $+ \times ?$ and $+ \times -$, the proportions were 91.4 per cent and 91.7 per cent. If all the progeny except those having positive parents are combined, positive diagnoses were recorded in 93.0 per cent. Doubtful cases were observed in 15.5 per cent and 16.9 per cent respectively.

As additional evidence of the genetic homozygosity of the mice of strain C58 MacDowell and Richter noticed that doubtful as well as non-leukemic offspring were scattered throughout the pedigree; not confined to one or more sublines. On the average non-leukemic individuals lived 100 days longer than animals which gave positive diagnoses.

The progeny of non-tumorous mothers of the C_3H or Z and the A strains have been tabulated in Table 2 to show the proportion of which developed mammary carcinoma. Both stocks have a high tumor incidence. In two classes the mothers had been mated to males from low-tumor stocks thus giving the incidence in the first generation hybrids. In another instance the females of the A strain which showed primary lung carcinoma are considered.

The total number of progeny observed by the author from non-breast tumor mothers was 94 and the mammary tumor incidence among them was 74.5 per cent. Of the 23 hybrid animals which had non-tumorous mothers from the high cancer strain 19 or 82.6 per cent developed breast tumors. Likewise the proportion of mammary tumors (70.6 per cent) among the progeny from mothers which had primary lung tumors is far greater than one would expect as due to chance alone.

These observations by Strong, MacDowell, Richter and Bittner show the value of using inbred strains of mice in cancer investigations and that the genetic constitution of all individuals—cancer as well as non-cancerous—within a pure stock is probably identical. Thus, animals which do not show cancer somatically in a high tumor strain of mice are potentially cancerous and must be considered as genetically cancerous. In mating these non-cancerous individuals all the evidence procured thus far from pure stocks demonstrates that they must be considered as cancerous parents.

Once inbred stocks have been established the next probable advance may be attained by outcrossing strains which differ markedly in their tumor incidence. That is, for example, a strain having a high incidence of breast cancer should be crossed to a race having little or no breast cancer. To maintain an absolutely non-breast tumor strain of mice is almost an impossibility and may never be attained. Several races have been observed after they had been inbred for the required number of generations to make the individuals homogeneous, for as long as ten years without the appearance of breast cancer, only to have one or two turn up within a short interval. Following the unexplained observation of these sporadic tumors no growths were recorded for another long interval. Whether the few tumors which were observed resulted from a proc-

TABLE 1

A classification of the offspring in strain C58 according to the presence or absence of leukemia in the parents

(Reproduced from MacDowell and Richter (1935), Table 4.)

PARENTS	OFFSPRING			PERCENTAGE OF DEFINITE DIAGNOSES POSITIVE FOR LEUKEMIA
	+	−	?	
+ × +	286	36	59	88.8
+ × ?	53	5	13	91.4
? × ?	11	0	1	100.0
+ × −	55	5	12	91.7
− × ?	4	0	0	100.0
− × −	10	0	3	100.0

TABLE 2

The proportion of the progeny from non-breast tumor mothers of the A and Z stock developing mammary tumors

STOCK	DIAGNOSIS OF MOTHERS	TYPE OF PROGENY	NO. OF PROGENY	PER CENT WITH BREAST CANCER
Z	Non-tumor	Z Stock	23	65.2
Z	Non-tumor	ZIF_1	17	88.2
A	Non-tumor	A Stock	31	77.4
A	Non-tumor	AXF_1	6	66.7
A	Lung tumor	A Stock	17	70.6
TOTAL			94	74.5

ess analogous to somatic mutation is problematical. Data tabulated for such a strain for certain periods might indicate that it was a non-tumorous stock.

In considering any experiment the above principles of genetics will be applied if the results are given a genetic interpretation.

In any review of the literature on cancer the work of Loeb, Tyzzer, J. A. Murray, and Lathrop and Loeb should be mentioned. Their work was conducted in the period during which genetics was in its infancy. Although inbred strains of experimental animals were not procured, their investigations furnished the basis for further experimentation and indicated that cancer susceptibility was inherited.

Another interesting experiment was published by Tureen and Loeb (1929). The degree of inbreeding of their strains of mice was not mentioned. Fourteen groups of hybrids were raised and were observed for tumor rate and tumor age. When the parents differed in these factors, the hybrids were somewhat intermediate in both tumor age and incidence; in other crosses it approached the age and rate of one parent more closely than the other. Either the female or the male parent might predominate.

Marsh (1929) also noted mammary tumors in the hybrids between high and low tumor stocks which had not been inbred. Likewise, the strains of mice used by Dobrovolskaia-Zavadskaia (1933) were not homogeneous.

A vast amount of work has been published by Miss Slye. Her theory as to the inheritance of cancer may best be expressed by a quotation from one of her latest publications (1933, p. 538):

> From consistent results obtained in this laboratory during twenty-three years and involving 116,000 autopsies, the general trend indicates two facts clearly:
>
> 1. The genetic difference between cancer susceptibility and cancer insusceptibility involved one gene; that is, these are unit characters.
>
> 2. In the stock in this laboratory, cancer susceptibility behaves like a recessive; insusceptibility like a dominant.
>
> There remains to be worked out (1) the problem of the interrelation of neoplasms of different types and of different locations, that is, what I shall call the "distributing factors"; and (2) the problem of the interrelation between the external factor, other internal factors, and the hereditary factors.

My definition regarding the use of analyzed stocks of mice in a genetic problem applies to this article. Quotations from Slye's reports will verify this statement:

> "On Dec. 2, 1910 (over twenty-two years ago), the original cross was made which resulted in strain 73. The parents were a silver-faun female purchased from Abby Lathrop, fancier of Granby, Mass., and a piebald grey-white male of strain 90, classic in these studies. None of the ancestry of this female was on record. The female herself died without autopsy, but there was no external evidence of tumor.
>
> "The parent male also died without autopsy. He too showed no evidence of tumor. His father, male 30, died with chronic nephritis without cancer. His mother, however, female 3, died of a mixed sarcoma-carcinoma of the mammary gland, malignant adenoma of the liver, and sarcoma secondaries in the kidneys from the sarcoma element of the mammary gland mixed tumor. Female 3 is also classic in these studies.
>
> "The parent male of strain 73 was, therefore, certainly at least heterozygous to cancer. Sarcomas, carcinomas, adenomas, and leukemic diseases were frequent in strain 90...." This extract is from the 32nd Report, p. 540 (1933).
>
> The parent male of strain 73 was "a piebald grey-white male of strain 90" (1933). Another designation for this male parent is: "a Japanese Whitefoot male" (1914, p. 286, and 1931, Chart 1).
>
> Female 3 and male 30 of strain 90 were the parents of the Japanese Whitefoot male used as the male parent of strain 73. In the 5th Report (1916, p. 495) we find that female 3 is a sister of male 30: "The significant progenitor here is female No. 3 of strain 90. Inbred with her brother, male No. 30...." The 28th Report (1931, p. 1366) denotes that the ancestry of female No. 3 is unknown: "....The ancestry behind female 3 is unknown to me, as she is the first member of the strain in my hands...."

Thus, we find that the ancestry of the female parent of strain 73, a silver-faun female secured from Lathrop, is unknown. On the male parent side the ancestry is known for one generation. Needless to say the genetic or neoplastic constitution of the two parents of strain 73 was almost a mystery. It might be interesting to summarize Slye's interpretation.

In the 2nd Report (1914, p. 501) we find regarding the parents of strain 73:

"Both parents came of cancer families but both died before autopsies were held, and it is uncertain whether or not they would have developed cancer." Later in the 3rd Report (1915, p. 165) strain 90, from which the parent male of strain 73 was descended, is described as . . . "a slightly tumorous strain of inbred grey-white piebald mice, carrying a low per cent of cancer." Another reference to strain 73 was given in the 16th Report (1921, p. 154): "Chart 9 shows part of strain 73, which was derived from the same female 3, mated this time with male 30. Male 30 came from a strain carrying tumors of the lung, mediastinum, and diaphragm, and was proved heterozygous to tumors of these organs, having been tested in various crosses." Strain 90 was also described in the 5th Report (1916, p. 482): "There have been in this strain malignant growths in the mammary gland, lung, liver, kidney, ovary, testicle and mediastinum."

Since the ancestry of strain 90 was unknown beyond female 3 and male 30, it is interesting to be advised regarding the heterozygosity of male 30 to various types of tumors, for, to my knowledge, no chart has been published by Slye showing that the descendants of this pair have been inbred for the required number of brother-to-sister matings to make the stock homogeneous. They were used, however, in hybrid crosses.

It might also be interesting to learn of the various types of cancer transmitted by female 3. In the 2nd Report (1914, p. 288) her condition was diagnosed thus:

". . . Female No. 3, had sarcoma of the mammary gland with metastases in liver and kidney. . . ." In the 5th Report (1916, Chart 5) female 3 was listed with "Sarcoma of the mammary gland and adenoma of the liver." Chart 7, same report, inserts the term "malignant" adenoma of the liver. The metastatic sarcoma of the liver and kidney was omitted.

By 1921 (16th Report, p. 141) the diagnosis for female 3 had become: ". . . The parent female, No. 3 had a sarcoma-carcinoma of the mammary gland, a malignant adenoma of the liver, and a metastatic sarcoma of the kidney. . . ."

In this same report (p. 146) credit is given to this same mouse as follows:

"In these strains, then, female 3 introduced:
"a. Primary carcinoma of the mammary gland, . . .
"b. Primary adenoma of the liver, . . .
"c. Primary sarcoma of the liver, . . .
"d. Primary sarcoma of the kidneys, . . .
"e. Secondary sarcoma of the kidneys, . . .
"f. Secondary sarcoma of the spleen, . . .
"g. Primary sarcoma of the mesentery, . . .
"h. Secondary sarcoma of the mesentery, . . ."

Since female 3 introduced such a variety of neoplastic growths, according to Slye, it is evident that her neoplastic constitution would be uncertain until after several generations of her progeny had been raised. It would be difficult to determine definitely the types of tumors female 3 introduced into the hybrid crosses owing to the nature of the matings; many of the mice used, like female 3, came from stocks of unknown ancestry.

Little (1928) has shown that the recessive theory of Slye does not fit the data as published in her reports. I do not believe, however, that Little should have attempted to explain Slye's data for mammary gland cancer on the assumption that the homozygous type was lethal. It might have been advisable had he taken the view of Haldane who, in an article on "The Genetics of Cancer" (1933), in which Slye's work is not mentioned, stated:

Besides the work described above, a good deal has been done on stock which was not genetically homogeneous. From this work it is clear that, while a tendency to spontaneous cancer is hereditary, it is not due to a single gene, dominant or recessive, and also that a particular localization of cancer may be hereditary. . . . But all work with genetically hetero-

geneous material is unsatisfactory, because any individual may die before reaching the cancer age, and no other individual will be of just the same genetical make-up. Hence really exact work is impossible.

It has been our experience in this laboratory that a large percentage of all tumors arising in mice are mammary gland carcinoma. They are very unusual in males. Slye (1926) objects to the use of cancer of the breast in tumor study because they are very uncommon in males. She further stated in 1928 (p. 83) that "Breast cancer susceptibility, therefore, cannot be sex-limited. . . ." As there are no limitations between male and female we may include her data on mammary gland carcinoma in any examination of her results.

I wish to tabulate briefly the progeny of a mating between two cancerous individuals from her reports. Before doing so I wish to make it clear that I have adopted the method used by Little (1928, p. 34) in determining the results:

"In these matings "non-cancerous" animals, both of whose parents were cancerous, can be fairly included. This is true because if cancer is a simple recessive, then all descendants of any two cancerous mice mated *inter se* are genetically cancerous regardless of their somatic appearance." That is, I have considered all the progeny of this cancer × cancer mating as being genetically cancerous and have tabulated them as being cancerous parents if they were mated, even though they did not develop cancer.

Miss Slye has objected to this procedure (1928, p. 84) in these words: "For if we assume . . . . that the offspring of double cancerous parentage must be potentially cancerous, and we count them as such, *we are already assuming what we are attempting to prove.*"

If we assume the single recessive factor "cc" to represent cancer susceptibility, according to Miss Slye's theory, a cancerous male or female, having the constitution "cc", mated to another cancerous male or female, also having the constitution "cc", would produce only progeny having, like their parents, the cancer susceptibility constitution "cc" in the F_1, F_2, ... F_n generation. Regardless of the somatic appearance of cancer or not, all the progeny are genetically cancerous and must be called cancerous parents if they are mated. They may not be called cancerous individuals without the diagnosis of cancer at autopsy but should produce only cancerous progeny. Recessive individuals are always homozygous (cancerous according to Slye's theory) and, according to the laws of genetics, must breed true.

In Chart 9, 7th Report (1916) Slye published the ancestry of strain 338.

This chart is reproduced in part (Chart 1). The parents of this strain were female 5417 and male 7736. In the ancestry we again find female 3 and male 30 mentioned above.

The parents of strain 338 are not descended directly from female 3 and male 30. If we consider mice 3 and 30 to belong to the first generation, female 5417 and male 7736 are representatives of the sixth generation. By brother-to-sister matings it would take five matings to obtain progeny of the sixth generation. To produce the parents of strain 338 thirteen matings were made, three of which were not brother-to-sister. In addition, No. 5417 and No. 7736 are representatives of different sub-lines making strain 338 an outcross between lines of heterozygous individuals.

This particular mating was selected as both parents were cancerous and produced a large number of offspring. Two of their progeny were also considered by Slye as producing a 100 per cent cancer strain of entirely different derivation, that is strain 338, branch V. This family was derived from a double cancerous parentage, female 8619 with two carcinomas of the mammary gland and male 8715 with an adenoma of the liver. . . ."

In this chart (Chart 9, 1916) female 5417 was reported to have had 2 carcinomas of the mammary gland, carcinoma of the pelvis and metastases in the lungs. The parent male 7736 died of a papilloma of the lung and arterial sclerosis. In Chart 14, 16th Report (1921) male 7736 was reported as having had an adenoma of the lung.

Strain 338 was thus derived from a double cancerous parentage. All the descendants of this mating were cancerous genetically, if not somatically, and should be counted as cancerous parents.

In Table 3 we have tabulated the descendants of female 5417 and male 7736 according to the matings of the F_1 progeny of this pair. The mice are grouped according to sex. The mammary gland

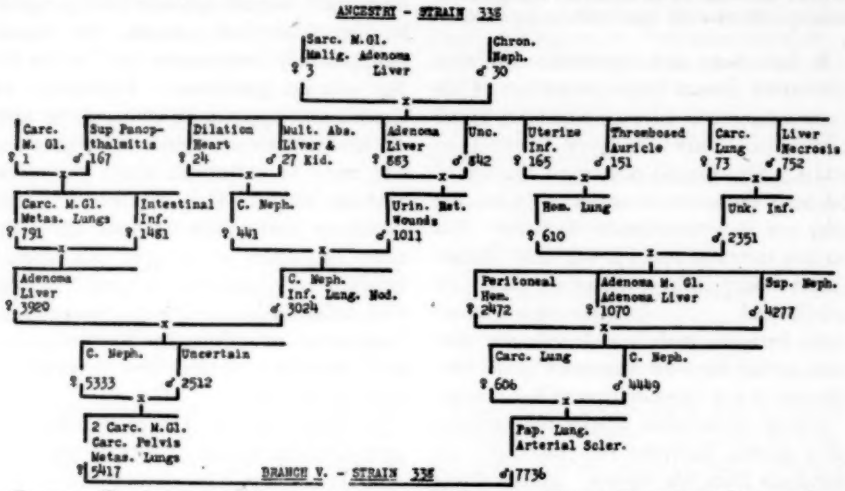

CHART 1. THE ANCESTRY OF ♀ 5417 AND ♂ 7736, THE PARENTS OF BRANCH V., OF SLYE'S STRAIN 338.
(Taken from Syle: 7th Report, Part of Chart 9 (1916))

TABLE 3

The descendants of cancerous female 5417 (carc. m. gl., carc. pelvis, metas. lungs) mated to cancerous male 7736 (papilloma or adenoma lung)

(The figures are from the data published by Miss Slye)

F_1 MATING	CHART	REPT.	CARC. M.GL.		OTHER TUMORS		TOTAL CANCER			TOTAL NON-CANCER			CANCER
			♀	♂	♀	♂	♀	♂	Total	♀	♂	Total	
													per cent
6993 × 6851	11	9	6	0	2	1	8	1	9	4	8	12	42.9
7899 × 6851	9	9	8	0	2	2	10	2	12	8	8	16	42.9
8619 × 5815	12	9	6	0	2	2	8	2	10	6	5	11	47.6
8619 × 5815*	11	7	1	0	1	1	2	1	3	1	2	3	50.0
8619 × 7394**	14	16	3	0	0	2	3	2	5	0	0	0	100.0
8619 × 8751	14	21	10	0	4	8	14	8	22	0	0	0	100.0
8619 × 8751***	9	7	12	0	1	2	13	2	15	5	14	19	44.1
9544 × 6441	10	9	4	0	4	1	8	1	9	3	7	10	47.4
TOTAL			50	0	16	19	66	19	85	27	44	71	54.5

*Does not include progeny given in Chart 12, 9th Rept.
**Does not include progeny from ♂ 9783 × ♀ 11560
***Does not include progeny given in Chart 14, 21st Rept.

carcinomas are listed separately but all other types of tumors are grouped together.

Female 5417 and male 7736 had 156 descendants which we were able to tabulate. As they were cancerous individuals all of their descendants should have been cancerous. There were 93 females, 66 of which had neoplasms or 70.97 per cent ± 3.14. Of the 63 males 19 or 30.16 per cent ± 3.99 had cancers. The difference between the sexes was 40.81 per cent ± 5.08 or 8.03 × P.E. The total number of individuals gave 54.49 per cent ± 2.66 cancerous mice. The difference between this observation and the expectation of 100 per cent tumorous, according to the recessive theory, was 45.51 per cent ± 2.66 or 17.10 × P.E.

For the benefit of the readers not familiar with probable error determinations, I might add that a difference greater than 4 × P.E. (1:142) denotes a significant variation from expectation in genetic work. The odds against a deviation of 5 × P.E. being due to chance are 1 to 1,341. Or expressed in terms of our ratio 8.03 × P.E. for the sexes, we should expect such a deviation, if Miss Slye's theory is correct, once in approximately 15 million times.

One will note from the table that two of the matings gave 100 per cent cancerous progeny. Chart 14, 16th Report (1921), giving the mating of 8618 × 7394, showed that other progeny were observed but not listed. The other mating was 8619 × 8751. In Chart 14, 21st Report (1926) all the progeny listed had neoplasms. In 1916 (Chart 9, 7th Report), ten years previous, other progeny are included, many of which are non-tumorous animals. The sex difference for the young descended from female 8619 and male 8751 was 5.34 × P.E., and the degree of significance for the entire group was 8.04 × P.E. when compared with the 100 per cent cancerous expectation. Thus, we observe that instead of producing 100 per cent cancerous progeny so few produced tumors that a significant difference was observed between the observed data and the expectation. While one does not expect 100 per cent cancer in any line, comparisons were made with this figure as Miss Slye stated that such were the data obtained.

If we exclude the carcinomas of the mammary gland, observed only in females, there is no significant difference between the sexes of the mice descended from female 5417 and male 7736. Since this type of cancer represented 76 per cent of all classes of tumor occurring in the females and 59 per cent of all tumors for the combined sexes, there is no apparent reason for not tabulating them. Rather it might be more important to consider a cross where only this type of cancer was observed.

TABLE 4

Types of matings of the first generation progeny of the cancerous parents 5417 and 7736 from Miss Slye's data

(References are given in Table 3.)

MATINGS	CANCER	NON-CA.	CANCER (per cent)
Cancer 6993 × Non-ca. 6851	8	11	42.1
Non-ca. 7899 × Non-ca. 6851	12	14	46.2
Cancer 8619 × Non-ca. 5815	12	13	48.0
Cancer 8619 × Cancer 7394	5	0	100.0
Cancer 8619 × Cancer 8751	35	19	64.8
Cancer 9544 × Non-ca. 6441	8	9	47.1

The data may also be considered according to the diagnosis of the progeny of female 5417 and male 7736 as has been done in Table 4.

Six matings were made. Although the parents were both cancerous not all of the first generation progeny were such. Two cancerous × cancerous matings gave 64.8

per cent and 100 per cent cancerous descendants. The latter mating gives only five progeny; Chart 14, 16th Report (1921) shows that some are not listed. The three cancer × non-cancer matings gave 42.1 per cent, 48.0 per cent, and 47.1 per cent cancer offspring. The single non-cancer × non-cancer cross gave 46.2 per cent cancer. One non-cancerous male, 6851, was mated twice to cancer female 6993 and non-cancer female 7899. The proportion of the descendants which were cancerous in the two matings were 42.1 per cent and 46.2 per cent respectively.

Cancer female 8619 was mated to three of her brothers. By non-cancer male 5815 she produced 48.0 per cent cancer, by cancer male 7394 all listed progeny were cancerous, and by cancer male 8751 64.8 per cent were cancerous.

The mathematical differences between Slye's results and the expectation of 100 per cent cancerous expectation might be explained by assuming that the non-cancerous mice died before reaching the cancer age. Granting as much, the recessive theory is still unable to explain the sex difference which was statistically significant as the theory states that mammary tumors are not sex-limited.

We may conclude that these data of Slye involving 156 descendants from cancerous parents do not support the theory that cancer susceptibility is dependent on a unit recessive factor. Since this article was written Miss Slye has altered her theory of cancer (Occasional Pub. of A. A. A. S., No. 4, 1937).

The status of the controversy has been ably summed up by Wells (1931):

> Obviously the genetic mechanism of the inheritance of susceptibility to cancer is still an unsettled problem, but data are now being accumulated in many places which should soon clear up many of the now contradictory contributions. . . . At the present time it seems safe to maintain that the existence of an hereditary influence on the susceptibility and resistance to cancer has been established both for man and animals. The exact mechanism of the hereditary influence has yet to be determined. . . .

In considering the genetic work on the inheritance of cancer it is evident that not all types follow the same principles of heredity. As most of the problems are on mammary gland, lung or leukemia, it seems advisable to tabulate each type separately.

MAMMARY GLAND TUMORS

Before reviewing the results on the inheritance of the susceptibility to breast cancer, a list of the inbred stocks of mice used will be given. Practically all matings were brother-to-sister or the progeny mated back to one of their parents.

High tumor strains

Dilute brown dba stock (Murray-Little). Inbred since 1909 or for more than 75 generations (Murray, 1934). More than 75 per cent of the breeding females living four months or longer develop carcinoma of the breast. Murray and Little (1935a) found that the incidence in virgin females was 50.8 per cent.

"A" Stock. A strain of albino mice started in 1921 by Strong (1936a). Fifty generations have been obtained by brother-to sister matings. From unpublished data the breast tumor incidence in breeding females was found to be 82.4 per cent (also Bittner, 1935c).

C_3H or "Z" Stock. Inbred since 1920 or more than 35 generations (Strong, 1935a and b). Of the breeding females living five months or longer 78 per cent develop mammary cancer (Bittner, 1935b).

"D" Stock (Strong). A sub-line of the dba strain. The incidence in breeding females was 56.3 per cent, average age 14.2 months (Bittner, 1936a).

Low tumor lines

C_{57} Black Stock. A strain of black mice inbred by Little since 1921. No breast cancers were observed among 240 breeding females by Murray and Little (1935a). Since that publication two have been recorded among several hundred additional breeding mice.

CBA or "X" Stock. Inbred since 1920 by Strong (1936b) who reported 2 tumors among 71 breeding females. In the author's line 13.5 per cent of 125

females have developed breast tumors, average age 21 months.

"N" Stock. A line obtained from Strong which has been inbred for 20 generations. The breast tumor incidence among 92 breeding females was 10 per cent, average age 17 months.

Mus bactrianus. Inbred since 1927. The tumor incidence in breeding females was about one per cent.

Yellow Stock. A line having a low incidence of breast tumors but a relatively high proportion of internal tumors (Little, 1934). Inbred since 1927.

"I" Stock. A race obtained from Strong in which no breast tumors had been recorded for twelve generations in a small number of progeny.

Zavadskaia brachyuric Stock. Representatives of this line were received from Dr. L. C. Dunn in 1931. This strain has not been closely inbred. Cloudman and Little (1936) observed one breast tumor in 52 virgin females.

Little in 1934 reported on the cross between the dilute brown stock and the yellow strain. Females from the high tumor line were used in making the first generation hybrids. The reciprocal cross was not made. The tumor incidence in hybrids according to the color of the mice was:

		Mammary tumors (per cent)
F_1 Generation		
Yellow mice	57	38.6
Non-yellow mice	54	64.8
Total	111	51.4
F_2 Generation		
Yellow mice	108	42.6
Dilute yellow mice	48	25.0
Brown mice	156	53.8
Dilute brown mice	67	46.3
Total	379	45.6
F_3 Generation		
All yellow mice	156	37.2
All non-yellow mice	223	51.6

Little found in the first and second hybrid generations that the yellow animals had fewer tumors than the non-yellow, the most probable explanation being the metabolic or physiological differences. Another important conclusion was that in a study of either the genetics or physiology of the incidence of spontaneous tumors, not all types of tumors or all colors of mice can be considered together.

A preliminary report of several crosses between high and low mammary tumor strains was published by the Staff of the Jackson Laboratory in 1933. In considering these observations reference will also be made to later publications where possible.

In these experiments when the maternal parents came from inbred high breast cancer lines, a high incidence of mammary cancer was also noted in their female descendants of the hybrid generations. In crosses where the maternal parents came from a low tumor stock the incidence of mammary tumors was significantly less in their female hybrid descendants. The chromosomal contents of the reciprocal hybrid mice were theoretically the same although the extra-chromosomal contents varied according to the maternal parent used in making the cross. In this preliminary report the theory of extra-chromosomal influence was advanced for the inheritance of susceptibility to mammary gland tumors, at least in mice.

Experiments supporting this theory are listed below:

dba × C_{57}Blk. Cross (Staff Jackson Lab., 1933, Murray and Little, 1935a).

Reciprocal crosses between the dilute brown high tumor and the C_{57}Black low tumor lines gave the following results in hybrid generations. Virgin females only were tabulated.

	No.	*Mammary cancer (per cent)*
dba♀ × C_{57}Bl♂ = dBF_1	113	39.82
C_{57}Bl♀ × dba♂ = BdF_1	356	6.05
dBF_1♀ × dBF_1♂ = dBF_2	664	35.54
BdF_1♀ × BdF_1♂ = BdF_2	687	5.96

These results have been verified by Korteweg (1934). Preliminary work by Murray and Little (1935b, 1936) has resulted in the following data for the back-cross generations. The approximate number in each class is 125 animals.

	Mammary cancer (per cent)	*Descended from*
BdF_1♀ × dba♂	7.14	Black mother
dba♀ × BdF_1♂	53.47	Dilute brown mother
BdF_1♀ × C_{57}Bl♂	6.00	Black mother
dBF_1♀ × C_{57}Bl♂	50.86	Dilute brown mother

dba High Tumor × *Mus bactrianus* Low Tumor (Staff Jackson Lab., 1933)

	No.	Mammary cancer (per cent)
dba ♀ × *M. bact.* ♂	69	68.11
M. bact. ♀ × dba ♂	27	7.41

"A" High Tumor × CBA or "X" Low Tumor (Staff Jackson Lab., 1933, Bittner, unpublished).

The incidence in this and the following crosses was for breeding F_1 females.

	No.	Mammary cancer (per cent)
A ♀ × X ♂ = AXF_1 breeding ♀	44	90.9
X ♀ × A ♂ = XAF_1 breeding ♀	16	18.8

C_3H or "Z" High Tumor × "I" Low Tumor (Staff Jackson Lab., 1933, Bittner, In Press).

	No.	Mammary cancer (per cent)
Z ♀ × I ♂ = ZIF_1 breeding ♀	36	91.7
I ♀ × Z ♂ = IZF_1 breeding ♀	10	0.0

C_3H or "Z" High Tumor × "N" Low Tumor (Bittner, unpublished).

	No.	Mammary cancer (per cent)
Z ♀ × N ♂ = ZNF_1 breeding ♀	48	97.9
N ♀ × Z ♂ = NZF_1 breeding ♀	18	27.8

"A" High Tumor × "D" High Tumor (Bittner, 1936).

	No.	Mammary cancer (per cent)	Aver. age Ca. (in months)
"A" Stock breeding ♀ ♀	421	63.9	12.3±0.13
A ♀ × D ♂ = ADF_1 breeding ♀ ♀	403	86.5	12.1±0.20
"D" Stock breeding ♀ ♀	207	56.3	14.2±0.20
D ♀ × A ♂ = DAF_1 breeding ♀ ♀	67	71.9	15.6±0.39

dba High Tumor × Zavadskaia brachyuric Low Tumor (Cloudman and Little, 1936).

In this outcross dilute brown females were mated to brachyuric males. Cloudman and Little observed the tumor incidence in virgin F_1 and back-cross females. The latter generation was made by mating F_1 males to C_{57} Black females.

	No.	Mammary cancer (per cent)
dba ♀ × Zav. bra. ♂ = dZF_1	163	76.7
dZF_1 ♂ × C_{57}Bl ♀ = BC	212	5.66

The experiments mentioned above are tabulated in Table 5. The incidence of mammary tumors in the F_1 generation females only is given.

SPONTANEOUS LEUKEMIA

Work on the inheritance of leukemia in mice has been reported by Slye (1931); MacDowell and Richter (1935), MacDowell (1935), and Richter and MacDowell (1935).

In the latter article we find regarding the work of Slye:

> Slye observed cases of mouse leukemia only in "tumor strains." She regards leukemia as a neoplasm, and believes that the genetic basis of all neoplasia is a recessive Mendelian gene.
>
> Three objections may be raised to Slye's conclusions.
>
> 1. The paper on leukemia presents only a portion of the cases observed (44 of 975). Complete data are necessary in this instance, for the conclusions are based on the *ex post facto* interpretation of pedigrees, rather than on experimental tests.
>
> 2. The hypothesis has not been tested by crosses between genetically homogeneous strains, as required for a genetic conclusion.
>
> 3. Genetic ratios are supposed to be indicated by the somatic condition of the mice—i.e. leukemic or non-leukemic. In view of the results obtained by MacDowell and Richter, mentioned below, the occurrence of leukemia does not necessarily indicate the genetic condition of the animal. For the determination of genetic ratios, breeding tests of individuals in a segregating generation are necessary.

The strains of mice employed by MacDowell and Richter in their leukemic studies were known as the C58 and the Storrs-Little stocks. Strain C58 has been inbred since 1920 or for over 25 generations by brother-to-sister matings. Approximately 90 per cent of the animals develop spontaneous leukemia of the various types. The Storrs-Little line has also been inbred for over 25 generations. The incidence of leukemia in this line was roughly one per cent.

The data resulting from the hybrid crosses conducted by MacDowell and Richter (1935) are listed on the following page.

TABLE 5

Various crosses between high mammary cancer and low cancer strains made by the members of the staff of the Jackson Memorial Laboratory

(Reference to each cross is given in the text. The incidence of mammary cancer in F_1 hybrid females is given in the last two columns.)

STOCKS CROSSED		HIGH CANCER STOCK			LOW CANCER STOCK		TYPE F_1 ♀	HIGH Ca ♀ × LOW Ca ♂		LOW Ca ♀ × HIGH Ca ♂	
High Ca	Low Ca	Gener. inbred	Ca. incidence		Gener. inbred	Cancer breeders		No.	Ca	No.	Ca
			Breed.	Virg.							
			per cent	*per cent*		*per cent*			*per cent*		*per cent*
dba	× Yellow	75+	80	51	15	10	Virg.	111	51.4	—	—
dba	× C_{57}Blk	75+	80	51	40	−1	Virg.	113	39.8	356	6.1
dba	× *M. bat.*	75+	80	51	20	1	Virg.	69	68.1	27	7.4
dba	× Brachyuric	75+	80	51	10	5	Virg.	163	76.7	—	—
"A"	× "X"	50	82	5	30	14	Breed.	44	90.9	16	18.8
"Z"	× "I"	30	78	?	12	?	Breed.	36	91.7	10	0.0
"Z"	× "N"	30	78	?	20	10	Breed.	48	97.9	18	27.8

	No.	*Leukemic (per cent)*
C_{58}♀ × Sto.-Li.♂ = F_1♀	86	56.2
C_{58}♀ × Sto.-Li. ♂ = F_1♂	80	68.2
Total	166	61.9
Sto.-Li ♀ × C_{58}♂ = F_1♀	77	40.6
Sto.-Li. ♀ × C_{58}♂ = F_1♂	63	45.2
Total	140	42.5

The difference between the incidence in the reciprocal hybrids was 19.4 per cent ±4.3 or 4.5 × P.E. In addition the hybrid animals which had mothers from the Storrs-Little stock lived 138 days longer than the other group.

The back-cross data to the Storrs-Little stock follows:

	No.	*Leukemia (per cent)*
F_1 ♂ (C_{58}♀ × Sto.-Li.♂) × Sto.-Li.♂	114	19.8
F_1♀ (C_{58}♀ × Sto.-Li.♂) × Sto.-Li.♂	188	46.5

The degree of significance between the results of the cross was 7.0 × P.E.

From these data MacDowell and Richter advanced the theory for the non-chromosomal maternal influence in the etiology of leukemic diseases in mice.

LUNG CARCINOMA

Tyzzer (1907, 1909) observed a larger proportion of lung tumors among the offspring of tumorous parents than tumor-free parents.

According to the recessive theory of Slye lung carcinoma would be inherited as a Mendelian recessive character.

Several reports on the inheritance to susceptibility to pulmonary tumors have been published by Lynch (1926, 1928 and 1931). The inbred low tumor strain 1194 was crossed to strain D having an incidence of approximately 40 per cent. The latter strain had been pen inbred and was, therefore, not very homogeneous. Females from strain 1194 were crossed to males from the D stock (1931). The proportion of F_1 hybrids living six months or longer which developed lung tumors was 24.4 per cent. First generation hybrid animals were back-crossed to individuals of the low tumor line 1194 and the resulting generation animals had a tumor incidence of 7.3 per cent. In the back-cross generation to the D strain the proportion of tumors was 32.2 per cent.

Lynch concluded that susceptibility to lung tumors was dominant or semidominant; the somatic development of tumors in animals which were genetically "tumor mice" was evidently prevented because of age and other influences.

DISCUSSION

From the work mentioned above it is evident that not all workers on the genetics of cancer susceptibility are securing similar results. The difference, in general, may be accounted for by the different methods of approach to the problem and the various genetic constitutions of the material used.

In the chemical preparation of a substance the chemist refuses to use chemicals which he knows in advance are not pure.

In the analysis of a genetic problem the investigator should consider the purity or the homozygosity of his material. To go further, not all chemicals which are white are the same compounds; not all mice which are white have the same genetic constitution and, unless they come from the same inbred stock, should not be considered as identical. This point is clearly brought out in the study of transplantable tumors by Tyzzer, Little, Strong and others. The use of animals produced at random—"market mice"—produced variations which could not be explained or duplicated. Mice of known ancestry from a single inbred strain, on the other hand, have given uniform results over a period of years.

Geneticists using inbred strains of mice have observed that the offspring from non-tumorous mothers of a high cancer line have as high a spontaneous tumor incidence as do progeny from tumorous mothers of the same race. Just as all the samples of a pure chemical are the same, so are all the individuals of a pure strain of mice identical.

The value of such strains for use in outcrossing is beyond question. The consistency of the results obtained by the use of various strains or different experimenters using the same material leaves little doubt regarding the significance of the observations. Although one worker states that the susceptibility of all types of neoplasia is inherited in the same manner, the work of others with more suitable material throws doubt on such a simple explanation. The cancer problem would probably be much simpler if all cancers had the same inherited susceptibility.

Seven outcrosses have been made by the members of the Staff of the Jackson Memorial Laboratory between high and low mammary tumor lines. Reciprocal F_1 generations were obtained in five of the crosses. In every cross mammary tumors were observed in the first generation hybrid females, either virgins or breeders. In every reciprocal cross the incidence of mammary tumors in the hybrid females was much higher when the mothers were from the high tumor line. To explain such results the extra-chromosomal hypothesis has been advanced. Additional evidence for such an explanation has been obtained by Murray and Little (1936) by controlled back-cross matings between the dBa and the C_{57}Blk strains. Korteweg (1934) has confirmed the evidence observed in the hybrid generations by using the same strains.

The work of MacDowell and Richter (1935) also shows that a non-chromosomal influence is involved in the inheritance of spontaneous leukemia in mice. The proportion of the offspring having this type of cancer was again higher when the mothers were from the high cancer line. The degree of significance was not as great, however, as in the above experiments which considered only mammary cancer.

In Lynch's experiments lung tumor susceptibility was observed to be transmitted through the male parent. The reciprocal cross was not made. A small number of animals observed by the author shows that it may be inherited through either parent.

Although susceptibility to mammary cancer and leukemia apparently follow the same type of extra- or non-chromosomal inheritance, one striking difference has been noted. That is, the incidence of leukemia in males approximates that of females. The chances of a male mouse having a mammary tumor is negligible.

Another distinction between these two forms of neoplasia may also be mentioned. MacDowell and Richter (1935) found that when the young from leukemic

parents were fostered by nursing mothers from a non-leukemic stock there was no change in the incidence among the fostered progeny. When young from mothers of the "A" high mammary strain are raised by mothers from low tumor lines the incidence of breast tumors in the fostered females, which were used as breeders, was significantly smaller than in the unfostered series of the same stock (Bittner, 1930b). This influence was transmitted to subsequent generations. Further work indicates that a "breast cancer-producing influence" may be transmitted in the milk of breast cancer mothers, offering an explanation for the extra-chromosomal theory of breast cancer. This work has been confirmed by Little using transplanted fertilized ova (Bittner and Little, J. Heredity, 28: 117, 1937) and by Andervont (unpublished) by the foster-nursing of the progeny from cancer mothers by low cancer stock females.

Primary lung cancer may occur in animals of either sex. No evidence has as yet been accumulated to date that it is extra-chromosomal in transmission.

From the data tabulated during the past ten years it is evident that some progress has been made in the field of cancer research in comprehending some of the details of the inheritance of susceptibility to some forms. Not all types of tumors may be grouped and studied in a single experiment; each type must be considered by itself. The problem is by no means solved but at least a start has been made by the use of homozygous stocks of mice and increasing interest in the problem by geneticists. Much of the controversy is due to the application of genetic principles to data not suitable for interpretation in this field.

The application of the findings in animal experimentation work to the human problem is problematical and remains for the future. The impossibility of securing human data to compare with inbred strains of animals where controlled matings may be made needs little comment. The reliability of considerable of the human data is questionable and leaves much to be desired.

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THE NUMBER AND MENDELIAN RATIOS OF PHENOTYPES AND GENOTYPES

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THE purpose of this paper is to offer a few convenient tables presenting the numbers and ratios of phenotypes and genotypes for any degree of hybridism. Although the results obtained are not new, the mode of presentation and the simple rules for deriving figures for any degree are, as far as the authors know, novel. Most textbooks on genetics, and many technical papers, treat only of low degrees of hybridism. Their method of presentation consists chiefly of square graphs (Punnett squares) or of configurations in the form of family trees, neither of which are convenient for determining high degrees. Exceptions to this approach will be mentioned in the body of this paper.

We therefore consider it desirable to publish exhaustive tables which designate phenotypic and genotypic ratios for any degree of hybridism. These tables furthermore correlate the results in a systematic manner and thus provide other information not readily seen in previously published expositions on this topic. The first two tables deal with phenotypes; the final table combines the mathematical relationships of phenotypes and genotypes. The accompanying explanation of the tables includes mathematical verifications of the relationships involved in the tables.

PHENOTYPES

In Table 1 the first row represents the number and ratio of monohybrids; the second row the number and ratio of dihybrids; and so forth. The numbers within the parentheses are the number of individuals displaying a given set of characters; the numbers outside the parentheses designate the number of types having the membership stated within the parentheses, and hence are coefficients of the numbers within the parentheses. The first column from the left represents the number of individuals displaying only dominant characters. Hence for every degree of hybridism there is only one such type. The second column represents the number of individuals displaying one recessive character and, by means of the coefficients, the number of types in a given degree of hybridism which display only one recessive character. The third column represents two recessive characters, and so on to the last column for each degree, where are to be found the number of individuals displaying only recessive characters. Therefore the coefficients on the uppermost diagonal are all 1. The column on the extreme right gives the theoretical total number of individuals.

To illustrate, let us consider row 4, which represents the tetrahybrids. The Mendelian ratio for this degree is

81:27:27:27:27:9:9:9:9:9:9:3:3:3:3:1

Theoretically, 81 individuals will show only dominant characters; four groups of 27 each will show one recessive and three dominant characters; six groups of 9 each will show two recessive and two dominant characters; four groups of 3 each will

show three recessive and one dominant character; finally one unit group shows all recessive characters. The theoretical total number of tetrahybrids is 256.

It is of interest that any row of this table can be analyzed by means of the technique evolved by Symbolic Logic in its algebra of classes. Each dominant character can be thought of as determining a class. Then the total number of dominant and recessive characters (= non-dominant) will be the total number of subclasses determined by the degree of hybridism; i.e. by the number of dominant characters being considered. It has been established that the total number of subclasses is 2^n, where n is the number of classes. In other words, we wish to make a full expansion of the universe defined by the number of dominant characters. Again the case of the tetrahybrid is used for illustration.

Let A, B, C, D be the four dominant characters and a, b, c, d the corresponding recessive characters. The following list exhausts all the possible combinations of male gametes resulting from the union of dissimilar parents. An identical list exhausts all the possible female gametes.

ABCD	aBCD	AbcD	aBcd
ABCd	ABcd	aBcD	abCd
ABcD	AbCd	abCD	abcD
AbCD	aBCd	Abcd	abcd

By mating each type of sperm to every type of ovum, one gets $16 \times 16 = 256$ types of the F_2 generation. From this list, the characteristics of mating each sperm to every ovum can be immediately determined, remembering that

$$Y \times Y = Y, Y \times y = Y, y \times y = y$$

It is immediately apparent that there will be exactly

1 case of only capital letters;
4 cases of exactly one lower case letter: a, b, c, d;
6 cases of exactly two lower case letters: ab, ac, ad, bc, bd, cd;
4 cases of exactly three lower case letters: abc, abd, acd, bcd;
1 case of exactly four lower case letters: abcd.

The number of cases are the coefficients of the fourth row of Table 1. Thus this method, which can easily be generalized for any number of characters, constitutes an analysis of each row.

The beautiful symmetry displayed by the table is worthy of note, for it permits one to extend it to any degree. The numbers within the parentheses of each column and their coefficients are terms of geometrical and arithmetical series respectively. The common ratio of the geometrical series is 3. The series of coefficients are arithmetical progressions. Thus, for instance, the coefficients of the fourth column: 1, 4, 10, 20, 35, 56, . . . can be reduced to 3, 6, 10, 15, 21, . . . (the coefficients of column 3), which in turn is reduced to 3, 4, 5, 6, . . . Interesting relationships can also be found by reading the table diagonally.

The following simple analysis is a further clarification and proof of the accuracy of the arithmetical progressions. The coefficients of column 2 represent the number of terms in a full expansion involving one recessive character. By a *full expansion* is meant here a list of terms such that every term contains each dominant or corresponding recessive character. The preceding list was a full expansion of the entire domain of four dominant and recessive characters. Column 2 is limited to one recessive character. Thus in the case of dihybrids we have Ab and aB;

trihybrids, ABc, AbC, aBC; etc. In short, there are two arrangements of one dominant and one recessive character, three arrangements of two dominant and one recessive, four arrangements of three dominant and one recessive character. In general, letting d be the degree, we have the general formula

$$C_1^d = d$$

which expresses the arithmetical progression of column 2; i.e. the combination of d things taken 1 at a time. Likewise the progression of coefficients in column 3 is

The *number* of phenotypes for any degree of hybridism is found by adding the coefficients of the row presenting the degree. However, it is clear that this number will be identical with the number of male gametes or of female gametes. Therefore the number of phenotypes will be

$$2^d$$

where d is the degree of hybridism. Note that $d = n$, where n is the number of dominant characters. This result also follows from the logical expansion given above in that the number of phenotypes is

TABLE 1

Phenotypes: increasing recessives

$(3)^1$	$(1)^1$								4
$(9)^1$	$(3)^2$	$(1)^1$							16
$(27)^1$	$(9)^3$	$(3)^3$	$(1)^1$						64
$(81)^1$	$(27)^4$	$(9)^6$	$(3)^4$	$(1)^1$					256
$(243)^1$	$(81)^5$	$(27)^{10}$	$(9)^{10}$	$(3)^5$	$(1)^1$				1024
$(729)^1$	$(243)^6$	$(81)^{15}$	$(27)^{20}$	$(9)^{15}$	$(3)^6$	$(1)^1$			4096
$(2187)^1$	$(729)^7$	$(243)^{21}$	$(81)^{35}$	$(27)^{35}$	$(9)^{21}$	$(3)^7$	$(1)^1$		16384
$(6561)^1$	$(2187)^8$	$(729)^{28}$	$(243)^{56}$	$(81)^{70}$	$(27)^{56}$	$(9)^{28}$	$(3)^8$	$(1)^1$	65536
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.

a series of full expansions of terms with two recessive characters. The general formula will be

$$C_2^d$$

i.e. the combination of d things taken 2 at a time. The formula for column 4 is

$$C_3^d$$

In general, the coefficient of any term in column 2 and onward will be

$$C_r^d$$

where r is the number of recessive characters.

the square root of the total number of individuals of a degree. Example: for tetrahybrids there are $2^4 = 256^{\frac{1}{2}} = 1 + 4 + 6 + 4 + 1$ phenotypes.

Bateson (3), Babcock and Clausen (2), and others have pointed out that the phenotypic ratio for any degree is an expansion of the binomial $(3 + 1)^n$, where n is the degree. Thus again to use the tetrahybrid as an example,

$$256 = (3 + 1)^4 = 1 \times 3^4 + 4 \times 3^3 + 6 \times 3^2 + 4 \times 3 + 1 \times 1.$$

In general [See Bateson, *op. cit.*, page 59; also Pearson (9), page 56, for determination of 4^n.]

$$
\begin{aligned}
4^n &= 3^n \\
&\quad + 3^{n-1} + 3^{n-1} + \cdots n \text{ times} \\
&\quad + 3^{n-2} + 3^{n-2} \\
&\quad + \cdots \tfrac{1}{2} n(n-1) \text{ times} \\
&\quad + 3^{n-3} + 3^{n-3} \\
&\quad + \cdots \tfrac{1}{6} n(n-1)(n-2) \text{ times} \\
&\quad \vdots
\end{aligned}
$$

This somewhat obscure statement of the general formula can be clarified by rewriting it as follows, remembering that one of the terms of the binomial is unity.

$$
4^n = 3^n + n3^{n-1} + \frac{n(n-1)}{1 \cdot 2} 3^{n-2} + \frac{n(n-1)(n-2)}{1 \cdot 2 \cdot 3} 3^{n-3} + \cdots
$$

This ingenious mathematical application gives the numbers in each row of our Table 1. However, the formula must be applied with care for we are dealing not with numbers but with phenotypes. Thus it will be observed that Bateson's third line in the original is inaccurate, for the ratio is

$$27:9:9:9:3:3:3:1$$

and not, as is given there

$$27:27:9:1.$$

The application of the propositions of pure mathematics is often obscure, whereas our tables prevent the emergence of such inaccuracies.

When the formula is interpreted phenotypically, it is of considerable value. Let the exponent of the binomial be the degree of hybridism and let the coefficients in the expansion represent the number of times the corresponding terms are repeated in the ratio. These coefficients correspond to our numbers outside of the parentheses. The addition signs have no significance phenotypically. The sum of all the terms in the expansion is the total number of individuals. It must be remembered that multiplication takes precedence over addition. The coefficients are always integers, for the numerator of each coefficient will be exactly divisible by its denominator.

Pearson's analogous statement of this expansion, in his already cited paper, is in some respects preferable to the direct use of the binomial $(3 + 1)$ in that he shows the genetic constitution of each term. This paper is so important to the topic at hand that we consider it necessary to present a part of it. The union of heterozygotes aA and aA give rise to the combinations aa, 2aA, AA. Letting u = aa, v = aA, w = AA, the union of the corresponding couplets results in

$$u + 2v + w.$$

Hence the general distribution for degree n will be

$$
(u + 2v + w)^n = u^n + nu^{n-1}(2v + w) + \frac{n(n-1)}{1 \cdot 2} u^{n-2}(2v + w)^2 + \cdots
$$

Basically this is the same as the binomial given above, for it is an expansion of the multinomial $(1 + 2 + 1)^n$.

By "protogenic couplet" Pearson refers to the combination or zygote AA; "allogenic" designates the couplet aa; and "heterogenic" signifies the couplets Aa or aA. "Thus, for example, there would be out of a total population of possibilities 4^n: 1 purely allogenic individual, $n \times 3$ individuals with $n - 1$ allogenic couplets; $2n$ of these would have one heterogenic couplet, and n would have one protogenic couplet." [Pearson (10) p. 57; Blaring-

hem (4) p. 293.] For instance, among the dihybrids there are two individuals with one protogenic couplet each, namely aa*BB* and *AA*bb; and four individuals with one heterogenic couplet each: aa*bB*, aa*Bb*, *aA*bb, and *Aa*bb. Each of these six individuals has one allogenic couplet. It can now be readily seen that each term in the expansion expresses the constitution of the individual. In the case of the dihybrids, the second term is $2w(2v + w)$. Of the six individuals represented by this term, four are generated by combinations of u and v; two by u and w. For a more complete analysis of Pearson's expansion, the reader is referred to the original. It is suggested that special attention be paid to the continuation of the above quotation and to the corollaries i and ii. This exceptionally important paper is well worth reading in its entirety. Lack of space alone prevents us from giving it the prominence in this article it so justly deserves.

We now proceed to Table 2 which is of considerable practical value and which results from rotating Table 1 so that the diagonals become columns. This tabulation makes apparent the relation of dominant characters and the numbers in parentheses.

Every instance of the number 1 represents a unit class containing individuals displaying only recessive characters; every instance of the number 3 represents classes of trios displaying exactly one dominant character; number 9 two dominants; etc. In short, letting d be the number of dominant characters (also the degree of hybridism):

$$3^d$$

represents the number of individuals in each class displaying d dominant characters. Note that $3^0 = 1$.

TABLE 2

Phenotypes: increasing dominants

							$(3)^1$	$(1)^1$	4
						$(9)^1$	$(3)^2$	$(1)^1$	16
					$(27)^1$	$(9)^3$	$(3)^3$	$(1)^1$	64
				$(81)^1$	$(27)^4$	$(9)^6$	$(3)^4$	$(1)^1$	256
			$(243)^1$	$(81)^5$	$(27)^{10}$	$(9)^{10}$	$(3)^5$	$(1)^1$	1024
		$(729)^1$	$(243)^6$	$(81)^{15}$	$(27)^{20}$	$(9)^{15}$	$(3)^6$	$(1)^1$	4096
	$(2187)^1$	$(729)^7$	$(243)^{21}$	$(81)^{35}$	$(27)^{35}$	$(9)^{21}$	$(3)^7$	$(1)^1$	16384
$(6561)^1$	$(2187)^8$	$(729)^{28}$	$(243)^{56}$	$(81)^{70}$	$(27)^{56}$	$(9)^{28}$	$(3)^8$	$(1)^1$	65536
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.	.	.

GENOTYPES

A group of individuals possessing the same genes belong to the same genotype. Thus there are three distinct genotypes in the case of the monohybrids: AA, Aa, aa, since, Aa = aA. Likewise there are 9 distinct genotypes in the case of the dihybrids, for there are three combinations of AA with BB, Bb, bb; three combinations of Aa with BB, Bb, bb; and lastly three combinations of aa with BB, Bb, bb. In short, the number of distinct genotypes is

$$3^d$$

where d is the degree of hybridism. Thus the number of genotypes is the same as the number of individuals in each class displaying d dominant characters. This is analogous to a preceding result which was that the number of phenotypes, 2^d, is identical with the number of dominant characters being considered in a given case.

As an illustration a table of genotypes (Table 3) for trihybrids is given. This is the familiar square graph representation. The numbers within the graph represent distinct genotypes. Note the perfect symmetry along the main diagonal.

Table 4 is based on Table 2, and contains the classes, ratios, and membership of both phenotypes and genotypes. Thus the table tells at a glance how many distinct genotypes are to be found in each phenotype for every degree of hybridism. The symbolism employed is identical with that of Tables 1 and 2, and hence needs no further explanation.

To illustrate. In the case of dihybrids, there is one phenotype theoretically containing 9 members. These are subdivided into one genotype of 1 member, two genotypes of two members each, and one genotype of 4 members. We shall now prove that this table can be extended to any desired degree of hybridism. Having already discussed the phenotypes, the present discussion is limited to genotypes. First we consider column 2 from the right.

Since the individuals in this column display only one dominant character, if G is any group of characters, the composition of G will have either one dominant gene or two dominant identical genes. In the latter case, the two identical genes will generate as many unit types as there are characters involved; i.e. degree of hybridism. In the former case, the dominant gene and its corresponding recessive will generate as many types of two members as there are characters involved. Therefore the genotypic items in column 2 will always be

$$(1)^d(2)^d$$

TABLE 3

A table of genotypes for trihybrids

	ABC	ABc	AbC	Abc	aBC	aBc	abC	abc
ABC	1	2	3	4	5	6	7	8
ABc	2	9	4	10	6	11	8	12
AbC	3	4	13	14	7	8	15	16
Abc	4	10	14	17	8	12	16	18
aBC	5	6	7	8	19	20	21	22
aBc	6	11	8	12	20	23	22	24
abC	7	8	15	16	21	22	25	26
abc	8	12	16	18	22	24	26	27

For instance, for monohybrids we have AA $(1)^1$ and Aa, aA $(2)^1$. Dihybrids: AbAb, aBaB $(1)^2$; Abab, abAb and aBab, abaB $(2)^2$. Trihybrids: aabbCc, aaBbcc, Aabbcc can be arranged into 3 types of two each $(2)^3$; AAbbcc, aaBBcc, aabbCC can be arranged into 3 types of 1 each $(1)^3$.

Analogous arguments verify the correctness of the other columns. Note that the ratio of the coefficients in each column is always the same. It is still necessary to prove that the series of numbers representing the individuals within each genotype proceeds as shown in the table; i.e. 1, 2, 4, 8, 16, . . .

By adding a new character, each class

TABLE 4

Phenotypes—Genotypes

PHENOTYPE																			$(3)^1$	$(1)^1$
GENOTYPE																		$(1)^1$	$(2)^1$	$(1)^1$
PHENOTYPE																	$(9)^1$		$(3)^2$	$(1)^1$
GENOTYPE															$(1)^1$	$(2)^2$	$(4)^1$	$(1)^2$	$(2)^2$	$(1)^1$
PHENOTYPE														$(27)^1$			$(9)^3$		$(3)^3$	$(1)^1$
GENOTYPE											$(1)^1$	$(2)^3$	$(4)^3$	$(8)^1$	$(1)^3$	$(2)^6$	$(4)^3$	$(1)^3$	$(2)^3$	$(1)^1$
PHENOTYPE										$(81)^1$				$(27)^4$			$(9)^6$		$(3)^4$	$(1)^1$
GENOTYPE						$(1)^1$	$(2)^4$	$(4)^6$	$(8)^4$	$(16)^1$	$(1)^4$	$(2)^{12}$	$(4)^{12}$	$(8)^4$	$(1)^6$	$(2)^{12}$	$(4)^6$	$(1)^4$	$(2)^4$	$(1)^1$
PHENOTYPE					$(243)^1$					$(81)^5$				$(27)^{10}$			$(9)^{10}$		$(3)^5$	$(1)^1$
GENOTYPE	$(1)^1$	$(2)^5$	$(4)^{10}$	$(16)^5$	$(32)^1$	$(1)^5$	$(2)^{20}$	$(4)^{30}$	$(8)^{20}$	$(16)^5$	$(1)^{10}$	$(2)^{30}$	$(4)^{30}$	$(8)^{10}$	$(1)^{10}$	$(2)^{20}$	$(4)^{10}$	$(1)^5$	$(2)^5$	$(1)^1$

on a given level generates *two* new classes on the next lower level; one through CC, the other through Cc (= cC). One of the new classes will be characterized by members having only dominant characters, and moreover it is evident that this is a unit class. The unit class, U, of the preceding level generates a new class of two members: UCc, UcC. Since every other class of a given degree has more than one member, the number of members of the new classes generated by the multiple-membered classes remain the same when combined with CC and are doubled when combined with Cc and cC. In brief, if there are m members in a given class, then by increasing the degree of hybridism by one character, there will be m members in the new classes characterized by CC, and $2m$ members in the new classes characterized by Cc. Hence with the addition of each character, the series representing the membership in the types is increased by one term, and the series itself is a geometrical progression with 2 as the common ratio. The difficulty of expressing this change is due to the fact that, upon addition of another character, complexities arise because of the uniqueness of CC.

Example. The first item from the left in Table 4 represents the monohybrids having at least one dominant character. There is one genotype of one member: AA, and one genotype of two members: Aa, aA. By adding a character B, new types are generated which are represented in the first item from the left, dihybrid row.

(1) AABB (2) AaBB (3) aABB
(4) AABb (5) AaBb (6) aABb
(7) AAbB (8) AabB (9) aAbB

Number 1 is the member of the unit type. Numbers 2 and 3 are members of a new type determined by A, a, B, B. Numbers 4 and 7 are members of a new type determined by A, A, B, b. The remaining four individuals constitute the fourth type. Therefore the series expressing the membership in these types is 1, 2, 4.

We are therefore permitted to extend the table to any degree of hybridism. If d is the degree of hybridism, there will be $d + 1$ terms in the membership progression of the first item from the left in any row. The progression is geometrical, with 2 as the common ratio.

It should be noticed that the coefficients of the genotypes in the first column from the left for each degree of hybridism are the same as the coefficients of the phenotypes for the entire degree. The reasoning behind this is the same as that behind the determination of the phenotypic coefficients.

The value of Table 4 lies in this, that it can be read for any degree of hybridism by simple extensions of the various series, which is obviously simpler than treating each degree separately.

With regard to the determination of genotypes, Babcock and Clausen (2, p. 98) state

> It may be noted that there is one phenotype in each class homozygous for all its factors. In this class starting with this phenotype, we double the number of individuals each time an additional pair of factors becomes heterozygous. . . . Thus (in the case of the trihybrids) there are three genotypes possible with only one heterozygous factor, and there will be two individuals of each of these, there will be three different genotypes having two heterozygous factors, and each of these will be represented by four individuals, and finally there is only one genotype with three heterozygous factors and it is represented by eight individuals.

It seems clear that Bateson and Babcock and Clausen logically and chronologically owe their approach to the topic of this paper to Pearson, although Bateson credits an anonymous mathematical friend for providing him with the binomial expansion. The mathematical relationships and conclusions of this elementary part of Mendelian theory could be derived from

Pearson alone. [de Vries (15) also considered the multinomial 1A + 2H + 1L, where A = active, L = latent, H = hybrid.] That still another discussion of it is here being presented is due to our belief that we have arranged the results in the simplest form yet published and because our tables offer completely the material they were designed to offer without mathematical exertion on the part of the reader. Nevertheless, we would like to point out that the discussions of the tables present, in some respects at least, a fresh point of view, and that our reference to Symbolic Logic as an instrument for the analysis of the theoretical relationships obtaining among genes is highly suggestive of the possibility of employing a new and powerful technique to the mathematical study of genetics. Woodger, in this QUARTERLY in 1930, pointed out a way of applying the modern logical tools to the analysis of biological concepts, while Serebrovskii (12) has enumerated and defined several operations for the algebraic development of a calculus of genes. There can be no doubt that a rich field awaits the logician who turns to an investigation of the mathematics of genetics.

[The authors wish to acknowledge their indebtedness to Prof. Raymond Pearl for his suggestions and criticism in connection with the preparation of this paper. Thanks are also due to our colleague Prof. John Paul Givler for urging that this paper be written and submitted for publication.]

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ESSAYS ON EVOLUTION

II. ON THE EFFECTS OF SELECTION ON SOCIAL INSECTS

By A. H. STURTEVANT

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THERE are two conflicting tendencies in recent developments of the mathematical theory of the behavior of wild populations. On the one hand there is the view that selection is ever-present and is the only effective agent in altering the constitution of populations. On this view selection operates only on individuals, and the members of a group are not thought of as occurring in semi-independent subgroups (Fisher 1930). On the other hand there is the point of view expressed by Wright (1932), according to which the most favorable conditions for evolution are those in which the members of a large population do not interbreed at random, but exist in more or less independent subgroups, which undergo exchanges of genes at times but are also effectively in competition with each other. On this latter view there is an opportunity, lacking on the first view, for selection to bring about the establishment of characteristics favorable to the group but unfavorable to the individual.

The social insects, such as termites, ants, and many bees and wasps, furnish examples in which such characters unfavorable to the individual have in fact become established. In terms of natural selection, a "favorable" character is of course to be taken as meaning a character that leads to the production of more descendants. The sterile castes of the insects named have, therefore, developed a character that is unfavorable by definition. It is sometimes argued that in this special case the rule still holds, since here the colony, rather than the individual insect, is the unit in terms of natural selection, and it may be supposed that the colony produces more offspring as a result of the division of labor associated with the sterile castes. It is clear, however, that all the social insects have arisen from solitary forms in which the sterile caste was absent (Wheeler, 1928). It follows that evolution must have resulted in an increase of sterile individuals. At some point in the history of the race there must have been a change from the individual to the colony basis of selection. Unless this change be supposed to have been a sharp one and to have been associated from the first with the necessary genetic adjustments, there must have been an intermediate stage in which some element other than the strict operation of reproductive selection was effective.

In fact, such intermediate stages still exist, and are familiar to all students of the social insects. In order that the colony be the unit in terms of natural selection, and that the existence of the sterile caste offer no difficulties to the advocates of pure selection, it is necessary that each colony have a single fertile queen and that the sterile individuals all be closely related to the queen (presumably her offspring). This is the standard account for the honey-bee, though even here it is

clear that workers may at times produce offspring; in other social forms wide deviations from this condition are frequent.

In what follows I shall confine myself to the ants, though evidently somewhat similar relations occur in certain of the termites at least. For detailed summaries of the life-histories and for bibliographies the reader is referred to Wheeler (1910, 1928) and to Donisthorpe (1915). Fertile workers do not appear to be rare among the ants; usually they produce only male offspring, but there are some satisfactory records of female offspring—the latter presumably due to failure of chromosome reduction in parthenogenetic eggs, since it seems clear that such fertile workers do not mate. The production of males is, however, enough to put these workers into reproductive competition with the queens; and therefore to upset the view that the colony is the unit of selection, to the exclusion of individual selection within the colony. Even more to the point is that, in many ants, more than a single fertile queen is present in a given colony. In such dominant genera as *Myrmica*, *Crematogaster*, and *Tapinoma*, and in the *rufa* and *sanguinea* sections of the genus *Formica*, flourishing nests regularly contain many fertile queens. In some (probably in all) these cases the colony is initiated by a single queen, but newly fertilized queens are adopted by established colonies after the mating flight occurs. In the case of *Formica rufa* at least, there is evidence that these adopted queens need not have been produced in the nest that adopts them—they may even belong to a distinct variety. Here and in *F. exsectoides* it seems probable that the colony long outlives the queen that originally established it, its fertility being dependent upon a whole series of queens that have no necessary genetic relationship to each other or to the founder.

Under these conditions there is evidently an opportunity for individual selection. A queen that produces unusually efficient worker offspring will have to share the benefits of their activities—i.e. her fertile offspring will have no advantage over those of the other queens living in the same nest. A queen that produces a relatively high proportion of fertile offspring will leave more descendants—at the expense of the nest economy, and in the long run to the detriment of the species. This possibility seems to constitute a serious danger to such ants, and may therefore be examined more closely.

There are ants in which the worker caste has disappeared entirely (*Anergates*, *Wheeleriella*, etc.), so that mutations of this sort are evidently possible. If such a one occurs, then a queen of the new type that is adopted by a normal colony will thrive at the expense of the worker offspring of her foster-sisters, but will produce more than her share of the sexual forms arising in the colony. Such a gene may be expected to spread through the population rapidly at first, but must soon be checked by the occurrence of too high a proportion of sexual forms to be supported by the reduced number of workers that will be available. It seems clear that the net result must be a decrease in the efficiency of the colonies and therefore of the total population of the species in the affected area. This will, evidently, leave such a region likely to be invaded by members of the species coming from places where such a process has not happened to occur.

Selection must, then, be thought of as operating, in these forms, on at least three different levels: on the individuals, on the colonies, and on the populations with-

in an area. The interrelations between these three are obviously complex. Similar distinctions apply in the case of another social animal—namely man. Here, of course, the situation is even more complex, and I do not have the intimate knowledge of the data necessary for a profitable discussion.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

TIMING LIFE

Being a review of *Biological Time* by P. Lecomte du Noüy. Foreword by Alexis Carrel. New York (Macmillan Company) 1937. $7\frac{3}{4} \times 5\frac{1}{8}$; xiv + 180. $2.00.

By Kurt Edward Rosinger, Woman's College, University of North Carolina.

Here indeed is an important book. Heralded two years ago by Carrel in his *Man, The Unknown*, it appeared late in the spring of 1937, rich with suggestion for intensive research by the mathematically inclined biologist or the biophysicist, fraught with interest for the philosopher who grounds his investigations in science, and tantalizing with speculative possibilities for the psychologist and sociologist. Nor is its central thesis, the determination of units for a biological time, of purely theoretical value. To the satisfactory solution of the problem herein presented accrue practical consequences of indubitable importance to the practicing physician. Because it proposes a many-faceted problem, even though it does not solve this problem, the book is important.

Although the author has divided his book into three sections, its subject-matter is logically divisible into two: (1) the statement and partial development of a purely technical and experimental problem in mathematical biology (Chapters IV-VII) and (2) a philosophical consideration of that problem and of its solution. This philosophical part is itself divisible into a preliminary presentation of the difficulties and limitations attending the application of a rigorous quantitative physico-chemical methodology to the domain of biology (Chapters I-III), and a philosophical deduction from the solution of the biological problem (Chapters VIII and IX). The former belongs primarily in the field of the logic of science, and would be of considerable value to the logician concerned with the methods of science. The latter is essentially metaphysical, being a brief analysis of the concept of time and of the differences between physical or sidereal time and biological time, a concept much discussed by Carrel and developed by Lecomte du Noüy. Like all metaphysical discussions, it bares itself readily to objections, and bases itself upon assumptions open to the critical attack of philosophers apparently far better trained in metaphysical technique than is the author. Yet it is doubtful that even these would cast sharp animadversions upon these two chapters for, although Lecomte du Noüy is at times naïve he presents in his concept of biological time an idea too rich in metaphysical possibilities to be subordinated to the pleasure of disparaging its promulgator. Nevertheless, this is probably the weakest part of the book.

The biological problem involves an attempt to discover an index of the rate of cicatrization of wounds and an index of the rate of proliferation of cells *in vitro*, a comparison of these indices, and as a consequence a correlation of these indices with a patient's calendar age to determine,

if possible, his age in terms of physiological units of time. The problem was first suggested to the author by Carrel when both were seeing military service in 1914. It was limited at first to the developing of a method for measuring the surface areas of wounds. By performing repeated measurements as the wound was in the process of healing, the rate of healing might be determined. If, moreover, it should be shown that this rate is a function of determinable variables, a general formula for cicatrization could be set up. Carrel had begun this study in a more or less non-quantitative manner, but precise mathematical methods were obviously indicated. These were to be supplied by Lecomte du Noüy.

At this point, one practical value of this research may be remarked upon. Satisfactory results would throw considerable light upon the nature and mechanism of the cicatrizing process, upon the relative merits of various dressings with respect to their inhibiting or stimulating influence upon healing, and might bring into relief the existence of other retarding or accelerating factors. The testing of antiseptics was, as a matter of fact, performed at an early stage in this research, with the result that Dakin's Solution Number 30 and Dakin's Chloramine-T were chosen from among almost two hundred substances at hand at the military hospital, as the most desirable.

It is beyond the scope of this review to give an account of the ingenious cerebrations through which the author arrived at his formula, nor the means employed in measuring the wounds and in constructing curves and tabulations for expressing the process of healing. Suffice it to say that after giving a clinical picture of the ordinary surface wound—knowledge possessed by the physician, but necessary information to readers without medical training—the author proceeds to present the steps through which he arrived at his formula. The excellence of the result is attested to by the fact that the difference in the time of the calculated and the actual period of cicatrization in a supposedly characteristic case was only two days over a period of ninety days; certainly an insignificant error under the circumstances. As an example of the quantification of biological methods, this account in *Biological Time* is well worth the efforts of any scientist or philosopher interested in this problem.

From here the author progresses to a further analysis of his formula and to what can be derived from it. An index of cicatrization, relatively constant for a given wound, had been discovered. The result now to be desired was to find a more general formula; one not limited to a specific wound. This the author proceeds to do, based on his observations that the index is a function of the area of the wound and the age of the patient. Small wounds cicatrize more rapidly than do larger ones, and those of young patients more rapidly than of older men. There appear to be no other relevant factors except inveterate alcoholism, diabetes, syphilis, and similar ones. From many experiments and from the study of curves describing the rate of healing, Lecomte du Noüy derived an empirical mathematical formula satisfying his needs. Hence the author claims that it is possible to determine the (physiological) age of a patient when the size of the wound is given and when it is known that other disturbing factors are absent.

The discussion of tissue culture *in vitro* is admirably clear and interestingly written, but is merely a brief review of material already to be found in the literature. Its value, in contrast to classical cytology, is well defended by showing that with this methodology one can observe cells active in their environment, and not dead upon a slide. Thus it is possible to study their reactions to modifications in the surrounding medium as a function of time. Moreover, knowledge of the physiology and pathology of cell life is greatly increased. But the result which is of particular concern to the topic of this book is that the growth index deduced from Carrel's experiments of cells *in vitro* is closely related to Lecomte du Noüy's own index of cicatrization. From these a new and distinct time, expressed in physiological units, can be derived.

As has been remarked upon above, the problem of determining time through the physiological activities of organisms has not been settled; it has rather been proposed and delineated. Much is yet to be done.

The experimental data must be greatly augmented. The possible relevancy of factors other than age and size must be investigated. The influence of the shape of wounds to the activity of cicatrization must be further studied. It may be well to know what effect upon the healing process the size of the wound has relative to the total surface area of the body. The mathematical application itself needs critical analysis. Many other facets of this problem exist which could occupy the attention of scientists, not the least of which is the search for other physiological processes whose measurement might aid in the determination of biological age, or might compel a revision of the concept.

Because of the scientific nature of this QUARTERLY, the philosophical sections of this book will not be reviewed in detail. The preliminary metaphysics expounded is neither original nor unusual. It is, however, plausible reasoning, based on a knowledge of physics with a Bergsonian inclination. It is well written but elementary. Of chief interest is the comparison of physiological to sidereal time. As is to be expected, the two are not congruent. Since at different calendar ages it takes different lengths of sidereal time to accomplish the same amount of cicatrization, it follows that in terms of physiological units, sidereal time apparently ceases to flow at a constant rate. Here relativity enters the domain of biology. However, the author prefers not to consider time as continuous, but rather as discrete. Time, like matter, electricity, and energy is granular, and "its continuity is only the statistical appearance given it by individuals."

A word should be said about the method of presentation employed in this book. Instead of being a statement of results and of interpretations of these, the author leads the reader along the steps of his cogitations, making of his research an attractive scientific adventure, although one in which only the technically trained reader can join.

BRIEF NOTICES

EVOLUTION

A STORY OUTLINE OF EVOLUTION. *By Chas. W. Grimes. C. P. Hoagland, Somerville, New Jersey.* $2.00. 7¾ x 5¼; 286; 1937.

This work is well named. Its extremely simplified language entitles it to be called a story and its lack of discussion of specific theories makes it a true outline.

The field of general evolution is an extremely wide one, for the doctrine first enunciated by the biologists has since been extended to include all the sciences and all the arts as well. An author who attempts to cover all of these must necessarily spread himself pretty thin. Consequently we find here no discussion of the factors of evolution, but only of the facts. The author has not always checked up on his statements, however, for he repeats the popular superstitions that the tadpole's tail is detached during metamorphosis, that snakes charm their prey with a hypnotic glance, and that witches were burned in America. Another feature not to be commended is the use of the word "vertebrate" to mean, not an animal with a backbone, but the backbone itself.

The author has been more successful in his treatment of cultural than in that of organic evolution. The chapter dealing with the evolution of instrumental music is perhaps the best in the book, but those dealing with the alphabet and the making of artifacts are nearly as good. The unification of the various fields is well accomplished. The blurb on the cover recommends this book as a gift for inquisitive youngsters, and with this opinion the present reviewer concurs.

DARWIN'S THEORY APPLIED TO MANKIND. *By Alfred Machin. With a Foreword by Sir Arthur Keith. Longmans, Green and Co., New York.* $3.00. 8½ x 5½; xxiv + 284; 1937.

In this sequel to *The Ascent of Man by Means of Natural Selection*, Mr. Machin finds expression for his twenty years' study and observation on the application of Darwin's theory of natural selection to mankind. Like the earlier volume, this

work is based on the author's firm conviction that if we believe that natural selection accounts for the origin and evolution of plants and animals, then we must believe that it accounts also for the evolution of man. Mr. Machin does not rest with a meagre explanation of his theory, but marshals forth fact after fact of undisputed physiological, psychological, biological and paleontological phenomena to prove his reasoning.

The author maintains that the instincts represent the original state of mankind, and that the other elements of our moral code have been developed in the process of evolution. As these elements have proved themselves either a benefit or a hindrance to the well-being of mankind, they have been retained or lost. Hence, modern man represents a bundle of survival values acquired from the different stages through which he has passed and is passing.

The book is well written in a clear and logical style, and will undoubtedly be stimulating to the mind of every deep-thinking biologist.

STRUCTURAL MODIFICATIONS IN THE HAWAIIAN GOOSE (NESOCHEN SANDVICENSIS). *A Study in Adaptive Evolution. University of California Publications in Zoology, Volume 42, Number 1.*

By Alden H. Miller. University of California Press, Berkeley. $1.00. 10¾ x 6¾; 79; 1937 (paper).

This study of the musculature and skeletal anatomy of the Hawaiian goose, including frequent comparisons to related North American genera, has as its object not only the description of anatomical features but more especially functional interpretation. The author undertakes a physiological anatomical analysis of limb mechanics, particularly hind limbs, in order to correlate certain characteristic differences in bulk of muscle with the habits and natural history of the species. The Hawaiian goose does less flying and swimming than any other species, and lives mostly on dry, barren, uneven, lava uplands. Therefore its limb structures represent considerable modification from the North American representatives of the genera. There are 115 characteristics differentiating this goose (*Nesochen sandvicensis*) from other species.

ORGANIC EVOLUTION PROVABLY FALSE.

By H. R. Kindersley. Thynne and Co., London. 1s. 7¼ x 4⅞; 49; 1937.

A vituperative attack upon the theory of organic evolution and its supporters, based on the argument of the immutability of breeding species; namely, that members of distinct species cannot, as a rule, interbreed and thus create new species. Experiments prove, the author claims, that species do not change, nor is there any evidence that new species emerge from established ones in natural ways. From this it follows that the theory of organic evolution collapses. Feeble stuff bred out of Ignorance by Emotion.

PALEONTOLOGY OF THE PLEISTOCENE OF POINT LOMA, SAN DIEGO COUNTY, CALIFORNIA. *Transactions of the San Diego Society of Natural History, Volume 8, No. 24.*

By Robert W. Webb. Society of Natural History, San Diego, Calif. 10¼ x 7; 12; 1937 (paper).

GENETICS

THE FIGHT FOR OUR NATIONAL INTELLIGENCE.

By Raymond B. Cattell. Introductions by Lord Horder, Major Darwin and F. P. Armitage. P. S. King and Son, London. 8s. 6d. net. 8¾ x 5½; xx + 166; 1937.

From the first to the last page of this book the author relentlessly pursues but one objective. This is to arouse the English people to the realization that the intelligence of the population is declining and that drastic preventive measures are necessary. The author presents three sets of facts. (1) The distribution of the I.Q.'s obtained on almost 3000 urban and almost 900 rural school children of 10 years of age. (2) A graphic representation of the distribution of the size of the sibship in the families of these children. This

graph indicates that on the average children with low I.Q.'s belong to large families and children with high I.Q.'s belong to small families. (3) A positive correlation of .73 between the I.Q.'s of parents and of children in 100 families. From these facts, mainly, the author reasons: intelligence is primarily inherited, people with low intelligence reproduce more children than those with high intelligence, *ergo*, in successive generations the percentage of people with low intelligence will steadily increase while that of persons with high intelligence will decrease.

Having reached this conclusion he proceeds to paint in very dark colors the future of England, its steady deterioration and subsequent destruction. Only eugenic measures can save the country from its fate, he says, and reviews in detail a number of plans all directed towards stimulating the reproductivity of persons with high I.Q.'s.

The author foresees that his conclusions may be criticized. So, in a manner verging upon political demagoguery, he already inveighs against future critics. At the risk of being called a "rhetorical advocate of *laissez-faire*", one cannot overlook the fact that the burden of proof still lies with him, and that the given data, in view of what is known regarding the inheritance of complex characters, do not wholly prove his conclusions.

THE ETIOLOGY OF MENTAL DEFICIENCY WITH SPECIAL REFERENCE TO ITS OCCURRENCE IN TWINS: *A Chapter in the Genetic History of Human Intelligence. Psychological Monographs, Volume 48, No. 4.*

By Aaron J. Rosanoff, Leva M. Handy and Isabel R. Plesset. Psychological Review Co., Princeton, N. J. $2.00. 10 x 6¾; 137; 1937 (paper).

This study is based upon the given case reports of 366 pairs of twins (126 monozygotic, 102 dizygotic and 138 pairs of opposite-sex twins), with mental deficiency in one or both twins of each pair. The data have been classified in various tables according to type of twin, sex, uncomplicated mental deficiency, mental deficiency accompanied by other mental disorders, and whether one twin is affected or both are affected similarly or dissimilarly. One section of the monograph is devoted to a discussion of sex factors in intelligence and in the etiology of mental deficiency, and in another the material has been further analyzed and discussed according to a suggested classification of the etiological factors of mental deficiency based on the periods in which these factors are operative: pre-germinal (hereditary), germinal, embryonic, fetal, intranatal, and postnatal factors. In the evaluation of their accumulated data and in their espoused theory of mental deficiency based upon it, the authors have remained cognizant of certain shortcomings in quality and amount of the raw material, but feel nevertheless that their findings hold promise as a basis for further intensive study of the problem.

TWINS: *A Study of Heredity and Environment. By Horatio H. Newman, Frank N. Freeman, and Karl J. Holzinger. University of Chicago Press, Chicago.* $4.00. 9¾ x 6¾; xvi + 369; 1937.

In this study a biologist, a psychologist and a statistician pool their intellectual resources in an attempt to produce a solution of sorts to the nature-nurture problem. They have not tried to derive a genetic-environmental ratio for the develment of the individual; their purpose has been merely "to secure evidence on the extent to which the characteristics of human beings, especially their ability and behavior, are determined by their genetic constitution and the extent to which these characteristics are influenced by the conditions of the environment."

Ten years have elapsed since the authors agreed to collaborate. During that time they have collected and analyzed three types of cases in far greater numbers than have ever before been studied. These types include 50 cases of identical twins reared together, 50 cases of fraternal twins reared together, and—the *pièce de resistance*—19 cases of identical twins separated in infancy and reared apart.

The problem is extremely complicated

and involves many factors. We have not space here to consider the inevitably complex (though not startling) conclusions at which the authors ultimately arrive. Suffice it to say that those interested in the problem will find much valuable material in this volume. The text is abundantly illustrated with tables and photographs and is adequately indexed.

ERBKRANKHEIT UND FERTILITÄT. *Micropathologie der Spermien erbkranker Männer.*

By H. Stiasny and K. D. J. Generales, Jr. With an Introduction by Erwin Gohrbandt. Enke Verlag, Stuttgart. RM. 27 (paper); RM. 29 (bound). 9½ x 6¼; xii + 163; 1937.

This is an important introduction to the study of the fertility of men afflicted with hereditary diseases, such as congenital imbecility, various types of insanity, epilepsy, blindness, deafness, and others listed under the German sterilization edict of July 14, 1933. It is devoted largely to the authors' own work, in the hospital "Am Urban" in Berlin, on analyses of spermatozoa of the types of men recommended for sterilization. In some hereditary diseases the number of normal spermatozoa found was only 37.15 per cent as compared with 81 per cent, on the average, among normal men. The greatest number of abnormal spermatozoa were found among the congenital idiots and the alcoholics. The idiots likewise had the lowest fertility rates (1 child apiece, on the average). The alcoholic groups, due to raised *libido*, had done better toward contributing to the census totals. Some information on the history and technique of sterilization, on normal spermatogenesis, and analyses after sterilization is included. The book is provided with illustrations, including 16 polychromatic tables. It contains a bibliography but is not indexed. It can well serve as a basis for further profitable research.

ANIMAL BREEDING PLANS.

By Jay L. Lush. Collegiate Press, Inc., Ames, Iowa. $3.00. 9¼ x 6¼; viii + 350; 1937.

The breeder of animals for economic purposes is still engulfed in a maze of practical difficulties for which science has as yet to offer solutions. But since the business of animal breeding must go on while further scientific knowledge is being acquired, it is well to bring together for study and comparison such methods as are at present available for improving the heredity of farm animals. Such has been the purpose of this author in writing a text for college students who have already had courses in genetics, embryology, anatomy and physiology of farm animals, herdbook study, history of breeds and stock judging. Genetic principles involved in animal breeding are reviewed, and breeding plans based on selection, relationship and somatic likeness are discussed pro and con. Other topics concerning breeding plans and relating to reproduction are considered briefly. Each chapter is summarized and followed by a list of references. The book includes subject and author indices.

CONTRIBUTIONS TO HUMAN HEREDITY. *In Honour of Maria Anna Van Herwerden.* Containing the following articles: *Die Bedeutung von Dr. Maria Anna van Herwerden für die Genetik und die Eugenik*, by Tine Tammes; *L'Examen Médical Prénuptial, ses Modalités et ses Consequences*, by G. Schrieber; *Ueber familiär-erbliche Fälle von seniler Makula-degeneration*, by P. J. Waardenburg; *Recent Progress in Blood-group Investigations*, by R. Ruggles Gates; *Erbliche Belastung in der Durchschnitts-Bevölkerung*, by G. P. Frets; *Eine Familie mit Kraushaar*, by J. Sanders; *Abortive Differentiation of the Ear Vesicles*, by Kristine Bonnevie; *The Coil-spring Properties of Chromosomes*, by Harry H. Laughlin; *Ueber eine verbesserte Methode der Berechnung der Abhängkeit von Merkmalen (Vierfelderrechnung)*, by H. Reichel.

Martinus Nijhoff, The Hague. Gld. 4. 10 x 6¾; 156; 1936 (paper).

The Editors of *Genetica* have honored the late Maria Anna van Herwerden by devoting two numbers of their journal to articles on genetics and human heredity. The eight papers appearing here represent

excellent pieces of scientific investigation, and mostly make definite contributions. Each paper is well illustrated and summarized, and the majority contain extensive bibliographies. There is a table of contents, but no index.

GENERAL BIOLOGY

MYSTERIES OF NATURAL HISTORY.

By E. L. Grant Watson. Frederick A. Stokes Co., New York. $1.75. 8 x 5¼; x + 244; 1937.

The readers of THE QUARTERLY REVIEW OF BIOLOGY do not need to be told that a vast number of biological problems have yet to be solved. Furthermore the normal reaction of these readers to this obvious fact is always to keep on working a little harder and more intelligently to answer the difficult questions, one by one. It is by persistently maintaining this attitude in the face of apparently insuperable obstacles and difficulties that science has made the progress it has in understanding and controlling natural phenomena. So long as priestly mysticism was the dominant attitude of man towards nature no such progress was made.

The patient preference of scientifically minded men for the precise clarity of real knowledge is not yet universal among mankind. To many soft-minded folk it still seems much pleasanter to look upon the facts of life as transcendental mysteries, and bemuse themselves in beautifully phrased speculations about the wonderful goodness and loveliness of it all. Some literary folk—and especially those a bit tainted with religious mysticism—have always been addicted to this type of intellectual masturbation. Drugged by their own verbiage into a gentle narcosis, they like to think that their lucubrations are setting bounds to science and to what it can know or teach.

The book that has led to these remarks is beautifully written, and has enjoyed a considerable popularity in England, both in magazine and book publication. The fact, however, is that its "absorbing style" is made the vehicle of an extremely subtle, and therefore the more vicious, undermining of biology and science in general. Space is lacking for more than one illustration of the technique, though various others and even more deplorable ones might be taken. The main point of a chapter entitled "Wise Worms," which deals with the behavior of the larvae of cerambycid beetles (which are *not* worms), is the fact that the pupae are generally so oriented that the head of each emerging imago will be pointed outwards, rather than towards the center of the tree. A very loosely constructed straw man is set up and then promptly knocked down again, to prove that natural selection as an explanation of this beautiful bit of teleology is, like most science, slightly comic. Then we are told that "the only alternative solution would be to postulate a life inspiration, a life wisdom, within the grub." Maybe so. But it will occur to biologists that some day a behaviorist and physiologist is surely going to do a competent and penetrating job of work on this problem, and may quite probably then find that the adaptation is *not* 100 per cent statistically perfect, and that its explanation when it does come off lies in a predictable and controllable tropistic reflex type of behavior in which neither larval "inspiration" nor "wisdom" play any discernible part.

Enough has been said to make it plain that THE QUARTERLY REVIEW OF BIOLOGY offers neither aid nor comfort to mystic purveyors of natural theology, however tastefully they may besauce their garbage or flavor it with the seductive elegance of their diction.

THE NATURE OF GROWTH. *A Logistic Inquiry.*

By Frederick S. Hammett. The Science Press, Lancaster, Pa. 75 cents. 9¼ x 6; 61; 1937 (paper).

This brochure is a series of numbered paragraphs, varying in length from one line to twenty-four, and in intelligibility, from not so much to none at all. We challenge those of our readers who have a taste for philosophical discussion to attempt the following:

217 If it may be assumed that in the beginning living matter was a formless loose collection of amino acids

in discontinuous conjunction with but a single activity of union and disunion—and if it may be assumed that a concatenation of conditions and happenings brought to such a gathering a group of other compounds wherewith combination occurred such as to produce a new property—that of pulsation or alternate contraction and expansion—possibly because of the endowment of the mass with colloidal properties capable of shifting in state with shift in environmental conditions—then along with this because of the properties of chemical elements in combination and molecules in aggregates there might have been such an oriented coacervation or organization of the condensate as would have produced the first and primitive muscle tissue—not because of the form but because the function of pulsation comprised chemical molecules in combination which took the form thereof

We have found, after careful research, that the author's excogitations first give clear indications of emerging from their obscure prose wrappings when the paragraph encasing a specimen is approximately nine lines. From that point on, scaling downwards, the increase in luminosity is very rapid and approaches an asymptote, as the following illustrates: "33 No single formula yet devised is adequate either to correlate the potencies already expressed or to predict those which the future may produce"

Chemical processes are referred to, biological terms are freely employed, and all without benefit (among other things) of much punctuation.

[*Editorial note:* Reginald the Office Boy, whose voice is now in process of changing, with the usual concomitants of greater seriousness of intellectual outlook generally and particular concern as to whether his collar is too dirty to wear one more day, likes this book. He says the only trouble is that biologists like the reviewer are just too dumb to understand a great book. Maybe Reginald is right. We venture no opinion, but the case does bring to mind Seneca's sage remark:

Quidquid excessit modum,
Pendet instabili loco.

Doctor Hammett's book does indubitably go beyond the present biological mode.]

ANIMAL COMMUNITIES IN TEMPERATE AMERICA *as Illustrated in the Chicago Region. A Study in Animal Ecology. Second Edition. By Victor E. Shelford. University of Chicago Press, Chicago.* $3.00. 9½ x 6½; xiii + 368 + 1 folding map; 1937.

In 1913 the early development of animal ecology was greatly stimulated by the original publication of this book. At that time there was a crying need for a rounded study of animal habitats. This was especially true since the plant ecologists were already well established and were rapidly outpacing their zoological colleagues. Shelford's book served the real purpose of collating and synthesizing a great deal of field knowledge. The approach was novel. Instead of dealing textbook fashion with communities drawn from all over the world, the author delineated a natural physiographic area and proceeded to treat it part-by-part as units comprising an ecological whole. The result was stimulating. A self-conscious animal ecology was born which, though frequently crude and inadequate, started a line of research and gave impetus to new activity. In short, a minor biological classic had appeared.

For this historical aspect one views this reprint of Shelford's book graciously in that it will permit younger students to get a copy for their shelves. It seems doubtful, however, if the book will prove of much professional value. This is unfortunate because it need not necessarily have been so. If, instead of merely reprinting the text, the author had modernized his views, corrected his errors and attempted to bring the volume in line with current standards the minor biological classic might have been elevated to a major one. As this was not done we can only feel grateful for partial favors and recommend the book in the same spirit that one might advise a youthful medico to purchase a first-edition of Gray's Anatomy.

GROWTH. *A Journal for Studies of Development and Increase. Volume I, Numbers 1–6, April, 1937.*

Subscriptions to be sent to the Secretary-Treasurer, Professor S. A. Courtis, School of Education, University of Michigan, Ann Arbor. $3.50 per volume of a minimum number of 400 pages (outside United States and Canada, $4.00). Single numbers 60 cents each. 9¾ x 6¾; 102; 1937.

This new journal is devoted to the dissemination of studies relating to any and

all aspects of growth phenomena regardless of the field of investigation. The first six numbers contain a paper by Davenport on a modification of the technique for measuring head features; one of a series of articles by Wetzel on a mathematico-physical theory of growth; a paper by Hammett and Schlumberger describing the effect of l-aspartic acid on the development of *Obelia*; a discussion by Northrup and Burr of an electro-dynamic theory of life; and a report by Goss and Asmundson on the glutathione and ascorbic acid concentration found in the carcasses of Barred Plymouth Rock and White Leghorn chickens of various ages.

In addition to its intention to cover the activities in a vast field, this new periodical has two distinctive features in editorial policy. In the first place, it is published by the coöperative efforts of authors and readers, the latter by subscription as is usual, the former by contributing to the cost of publication to a greater extent than is generally required. In the second place, the editors promise not to pass judgment on the merits of a work submitted. This policy may be instrumental in assuring the success of the new enterprise. Then again it may not.

KONGRESS FÜR SYNTHETISCHE LEBENSFORSCHUNG. *Verhandlungsbericht über die Aussprache zwischen Ärzten, Biologen, Psychologen und Philosophen in Marienbad vom 16. bis. 18. September 1936.*

Edited by M. Sihle and E. Utitz. J. G. Calve'sche Universitätsbuchhandlung, Prag. Kr. 10. 9¼ x 6; 208; 1937 (paper).

The volume gives the report of a congress for the promotion of medical synthesis held at Marienbad in 1936, and attended by prominent physicians, sociologists, psychologists, and philosophers. It contains essays on the following subjects: Synthetic thinking in research, by M. Sihle; Mnemistic biology and psychology, by E. Bleuler; Medical regulations in psychology, jurisprudence, sociology and political science, by E. Stransky; What service can philosophy render to the physician?, by O. Kraus; Brain pathology and personality, by H. Hoff; Life and spirit, by E. Utitz; The holistic investigation of biological phenomena, by L. v. Bertalanffy; Organ and organism under pharmacological influences, by E. Starkenstein; Psychotherapeutic syntheses in medicine, by Th. Bovet; General biology and medicine, by J. Bělehrádek; The present status of genetics in biology, by G. Wolff; The need for a world-view philosophy and philosophical psychology, on forms of regression, and psychotherapy, by M. Löwy.

The publication of this interesting report was made possible by the Josiah Macy, Jr. Foundation of New York, a subsidy that puts all theoretical biologists in its debt.

CONTRIBUTION A L'ÉTUDE DES RÉSERVES NATURELLES ET DES PARCS NATIONAUX.

By A. Aubreville, A. Barbey, E.-N. Barclay, C. Bressou, P. Chouard, J. et M.-L. Dufrénoy, F. Evrard, A. Feuillée-Billot, J. Fudakowski, H. Humbert, L. Joleaud, A. Joubert, L. Lavauden, P. Marié, G. Petit, P. de Peyerimhoff, E.-G. Racovitza, M. Tallon, C. Valois, V. Van Straelen and P. Vayssiére. Paul Lechevalier, Paris. 100 francs. 10 x 6½; 267 + 46 plates; 1937 (paper).

The Société de Biogéographie of Paris has set for itself the problem of finding the most reliable and most acceptable method of dealing with the problem of the conservation of natural things. The study, of which this monograph presents progress of findings to date, is a coöperative enterprise in that it represents the combined contributions of more than 20 workers.

The method used for making comparative studies of Natural Reserves and National Parks has been to find (1) the reason for the creation of the reserve or park; (2) the methods or techniques used in making the proposed reserve or park become a reality; (3) the results of periodic observations concerning the evolution of the fauna and flora of a reserve or park; and (4) the effect of immediate and surrounding influences upon the fauna and flora.

The work includes extensive studies of the parks and reserves of Europe, Asia, Africa and North America. The volume

is well illustrated with a number of fine photographs. There is a summary of topics discussed, but no index.

DESERT NEIGHBORS.

By Edith M. Patch and Carroll L. Fenton. Drawings by Carroll L. Fenton. The Macmillan Co., New York. $1.75. 7¾ x 6¼; vi + 170; 1937.

Here we have a work intended for children but which will appeal to adults as well—especially to the man who loves the desert well enough to want to spend his week ends on it, and to fry his eggs on the shovel over a fire of ocatilla wood, and sleep under the stars in the fragrance of the cresote bushes and the juniper trees, with the coyotes howling his lullaby and the quails singing his aubade—in short, it will appeal to all those who can say with the poet, "Desert, I love thy wan and dusty face."

The illustrations are well drawn, both of animals and of scenery. One in particular of a landscape in which an arroyo is the chief feature is so inviting that the reader will want to go walking in it, looking for petrified wood, fossil coral, and gypsum crystals, and make the acquaintance of Churca and Yodeler and Crota and Bannertail in their own homes, under the chollas and sahuaros and Biznagas.

A GRAPHIC SUMMARY OF PHYSICAL FEATURES AND LAND UTILIZATION IN THE UNITED STATES. *U. S. Department of Agriculture, Miscellaneous Publication No. 260.*

By O. E. Baker. Government Printing Office, Washington. 10 cents. 9¼ x 6; 57; 1937 (paper).

This pamphlet, arranged in two sections, consists of seventy maps and graphs based on census reports and the annual estimates of the Bureau of Agricultural Economics. The graphs in the first section deal with the physical conditions in the various states, including temperature, duration of frost season, moisture, topography, and the various kinds of soil in each section which are influenced in their development by climate and natural vegetation. The second section shows how the land is being used in each state. Maps indicate crop, forest, and pasture land, dry land that has been irrigated, and swamp land that has been drained. Crop failures as well as those harvested are indicated.

There is a complete index.

CONCORD RIVER. *Selections from the Journals of William Brewster.*

Edited by Smith O. Dexter. Illustrated by Frank W. Benson. Harvard University Press, Cambridge. $3.50. 9 x 6¼; vii + 259 + 12 plates; 1937.

This book is actually a sequel to *October Farm*—a diary of daily wanderings around a country home. It includes selections Mr. Dexter made from Brewster's diaries that were not included in the first publication.

Wherever Brewster went, whether merely a few steps from his door or for an overnight trip in his decked canoe, he saw birds before all else. These he described in natural and simple language that brings the animal life around Concord River into a living picture. Although the selections in this book are of the same type as those in *October Farm*, Mr. Benson's twelve excellent bird studies have added a new touch to *Concord River*. Three of these are in color, one is an etching, and eight are charmingly done in black and white wash. All stand out as different, realistic and exceptionally artistic.

There is a foreword by Thomas Barbour and a detailed index.

MITOGENETIC ANALYSIS OF THE EXCITATION OF THE NERVOUS SYSTEM.

By A. G. Gurwitsch. N. V. Noord-Hollandsche Uitgeversmaatschappij, Amsterdam. F. 3.75 (paper); F. 4.75 (cloth). 9⅞ x 6¼; 114; 1937.

This book is a theoretical outline of results obtained on work on mitogenetic phenomena of the nervous system. Part I deals with the radiation of the elements of the nervous system; Part II with the

effects of mitogenetic radiation on the nervous system. Some very interesting results are obtained, which are interpreted to show that the present concepts of nerve stimulation must be altered. It is claimed that spectrum analysis reveals the qualitative variability of the states of excitation of the nerve fiber and that its results refute the all-or-none law. Many other new ideas are advanced.

TESTS IN BIOLOGY.

By Frederick L. Fitzpatrick. Houghton Mifflin Co., Boston. 40 cents. 11 x 8½; 32; 1937 (paper).

These tests will certainly be appreciated by lazy teachers and by those who are overworked and have classes larger than they can handle properly. For the most part they consist of true-or-false questions, sentences with words to be filled in, and sentences where the student is to choose which of several indicated words makes a true statement. This makes it extremely easy to grade the papers, particularly since the author is kind enough to supply the teacher with a set of the "correct" answers. We should not be inclined to give the author a grade of 100 per cent. Many of the questions are so worded that no answer in the required form can be strictly correct and some others are petty if not positively silly.

EFFECT OF TEMPERATURE, HUMIDITY, AND OTHER FACTORS ON HATCH OF HENS' EGGS AND ON ENERGY METABOLISM OF CHICK EMBRYOS. *U. S. Department of Agriculture, Technical Bulletin No. 553.*

By H. G. Barott. Government Printing Office, Washington. 10 cents. 9 x 5¾; 45; 1937 (paper).

Over a period of four years the author conducted a series of careful experiments for the purpose of obtaining the optimum conditions for the hatch of hens' eggs and normal development of embryos. These conditions of temperature, humidity, and of oxygen and carbon dioxide content are presented to poultry raisers in an effort to reduce "an annual monetary loss of at least $14,000,000". After a brief review of other literature, there is a description, with photographs, of the respiration calorimeter used in the author's own experiments. Results are well depicted by frequent graphs and tables. The bulletin contains a table of contents and summary, but is not indexed.

BUGS, BIRDS AND BLIZZARDS IN THE YELLOWSTONE.

By Harlow B. Mills. Collegiate Press, Ames, Iowa. 50 cents. 9 x 6; vii + 76; 1937 (paper).

A naturalist here reveals to his audience the high spots among his recollections of a year spent on the range in the Yellowstone National Park. The birds of this forest reserve were a special delight to the author but his descriptions are mostly too fragmentary to convey his emotions to the reader. Most interesting is the story of a skiing trip taken around the "loop" of the park when deep snows and blizzards have made the country an isolated wilderness, but a place of beauty and charm in which some forms of wild life are still in evidence.

ECOLOGY IN TOWN AND CLASSROOM.

By R. Bracher. J. W. Arrowsmith, London. 2s. 6d. net. 7¼ x 4¾; 96; 1937.

This is a companion volume to *Field Studies in Ecology*. Whereas that book dealt with natural types of flora this deals with what are called artificial, since they have in one way or another been influenced by human beings. Studies were made and lists compiled of plants found in the following habitats: (a) streets, (b) waste ground (building sites, neglected gardens, rubbish dumps), (c) coal tips (with other artificial substrata), (d) fresh water communities (ditches, etc.), (e) tidal river banks and dockland.

ÖKÖLOGIE ALS WISSENSCHAFT VON DER NATUR *oder Biologische Raumforschung. Bios, Band VII.*

By Karl Friederichs. Johann Ambrosius Barth, Leipzig. RM. 8. 9¼ x 6; vii + 108; 1937 (paper).

The term ecology as used in this book does not denote a subdivision of biology, but a unified bio-centric science of Nature as a Whole, a Cosmos. The author makes a strong plea for a synthetic (organic, dynamic, holistic) study and interpretation of natural phenomena and their relations, man included, yielding not only knowledge but wisdom, and seeing in Nature a revelation of God.

GENERAL BIOLOGY. *Revised Edition.*

By S. J. Holmes. Harcourt, Brace and Co., New York. $3.50. 8½ x 5½; vii + 467; 1937.

This entirely rewritten edition of a well and favorably known text has a new chapter on elementary chemistry, useful to beginning students of biology, added at the beginning.

HUMAN BIOLOGY

DAMIEN THE LEPER.

By John Farrow. Sheed and Ward, New York. $2.50. 8¼ x 5½; xx + 230; 1937.

On the shore near the Kalaupapa landing in Molokai there stands a granite shaft bearing the inscription "Damien de Veuster. . . . Greater love hath no man than this, that a man lay down his life for his friends"—a strangely inadequate tribute to one whose entire life, like that of the original Author of these words, was a demonstration of a still greater love, one that impels a man to lay down his life for those who by no stretch of the imagination can be called his friends, yes, even for his enemies.

Father Damien really laid down his life sixteen years before physical death finally overtook him, for when he entered on his ministry on Molokai he knew that he would never be able legally to leave the island, and that it would be only a question of time until even his vigorous frame succumbed to what in ancient Egypt was known as the death before death. And at this time none of the unfortunate victims with whom he threw in his lot could be called his friends, for none of them had ever heard of him—in fact, his life was attempted by voodoo practitioners, and the lepers themselves arose in open revolt against him. But Damien was not one to turn back after having put his hand to the plough, with the result that when he was finally called to his reward as an old man at forty-nine, blind, paralyzed, and nauseated by the stench of his own decomposing flesh, he left behind him the devotion and love of his parishioners, the gratitude and admiration of intelligent people everywhere, and a reputation that defies the flight of time. He was like the bush that Moses saw on the desert of Midian—burning but not consumed, and the historian who contemplates his biography feels as Moses did, that he is standing on holy ground. Like Lincoln at Gettysburg, he feels the futility of words to add anything to deeds, and that about all he can do is to recount the facts.

In this instance, the author has not been altogether happy in the choice of facts he has chosen to emphasize. He is a member of the same denomination as Father Damien, and is quite justly proud of the record of his co-religionist. But this tends to make him somewhat unfair to those who adhere to creeds other than his own. He seems resentful of the fact that when the first permanent Roman missionaries landed on the islands under the protection of the guns of the French navy they found the field occupied by New England Protestants, who had been for about a decade unobtrusively but effectively laying the foundations on which later missionaries were to build. He also points out that the mob that revolted against Damien was led by Calvinists and Mormons, a statement perhaps true but certainly not necessary. Finally, he repeats the Hyde letter, which should have been consigned to oblivion long ago, and probably would have been, had a copy of it not fallen into the hands of Robert Louis Stevenson, a member of the same denomination as Mr. Hyde, who felt moved to reply to it—as if Damien needed vindication! The result has been the perpetuation of an unsavory controversy that would better have been forgotten. One

feels that if Hyde had been a Romanist Mr. Farrow would have passed him over in the silence he merits.

Apart from these details the book deserves a great deal of praise, for it is a scholarly piece of work, as the great number of acknowledgments at the beginning and the extensive bibliography at the end bear witness. Two unexpected chapters add tremendously to the force of the book—one is a history of the disease since the earliest mention of its occurrence in the valleys of the Nile and Euphrates to the present day, and the other a biographical sketch of Brother Dutton, Damien's able assistant, the revelations in the latter chapter being somewhat startling, in view of the fact that Brother Dutton is still living, or was until recently.

The story is well told, and the literary style of the writer grips the reader and holds his attention till the book is finished. Perhaps the most dramatic incident in the life of the hero is the confession that he made on the open ocean from his canoe to a priest standing in the stern of a passing steamer. The captain had stopped the ship for the occasion, but he had been given strict orders not to allow anyone to leave or board the ship at Molokai. Under these circumstances it was impossible to keep the confessional secret, but it is to the credit of the crew and the passengers that not one of them ever divulged anything which they might have heard.

The book is provided with a complete index of four pages, and a pen and ink drawing of Father Damien by Jean Charlot for frontispiece.

A Brief Rule to Guide the Common-People of New England. *How to Order Themselves and Theirs in the Small Pocks, or Measles. Publications of the Institute of the History of Medicine, The Johns Hopkins University, Fourth Series, Bibliotheca Medica Americana, Volume I.*

By Thomas Thatcher. Facsimile Reproductions of the Three Known Editions with an Introductory Note by Henry R. Viets. The Johns Hopkins Press, Baltimore. $1.50. 7¾ x 5¼; liv + 16; 1937.

A Discourse upon the Institution of Medical Schools in America. *Publications of the Institute of the History of Medicine, The Johns Hopkins University, Fourth Series, Bibliotheca Medica Americana, Volume II.*

By John Morgan. With an Introduction by Abraham Flexner. The Johns Hopkins Press, Baltimore. $2.00. 7¾ x 5½; vii + xxviii + 63; 1937.

Adaptation in Pathological Processes. *Publications of the Institute of the History of Medicine, The Johns Hopkins University, Fourth Series, Bibliotheca Medica Americana, Volume III.*

By William H. Welch. With an Intro- by Simon Flexner. The Johns Hopkins Press, Baltimore. $1.50. 7¾ x 5¼; xi + 58; 1937.

The first of these volumes (of a new series of the publications of the Institute of the History of Medicine of the Johns Hopkins University, *Bibliotheca Medica Americana*), is a facsimile reproduction of the earliest medical document printed in America, north of Mexico. In a frontier community such as seventeenth century New England where physicians were few, the clergy often ministered to their parishioners' bodily as well as spiritual ills. When therefore an epidemic of smallpox broke out in Massachusetts in 1677, the Reverend Thomas Thatcher, minister of the Old South Church, prepared a broadside, based upon Sydenham, describing the symptoms and treatment of the disease. This was reprinted during the epidemic of 1702 and again in 1721–22. These reprints are here reproduced for the first time. A brief life of Thatcher, a discussion of smallpox and other epidemic diseases in England and New England, and a bibliographic discussion of the three editions are also included.

The second volume is a facsimile reproduction of the address in which the first professor of medicine in this country set forth his views on the proper organization of a medical school. In it he emphasized the importance of a thorough grounding of the medical students in anatomy, chemistry and physiology, as well as the need of hospital facilities for their clinical instruction. He realized also that the system then customary in this country, by

which the same person prescribed and compounded medicines, was less efficient than a system in which the physician and the pharmacist might each have opportunity to develop his specialized knowledge.

The third volume is a reprint of Welch's presidential address of 1897 before the Congress of American Physicians and Surgeons. The basis of pathological adaptations, Welch pointed out, is to be sought in the physiological properties of cells. Thus the compensatory hypertrophy of the heart is a special case of the hypertrophy of a muscle required to perform prolonged extra work, "a capacity which has abundant opportunities for exercise in normal life, when the influence of natural selection and other factors of evolution can exert their full power."

NEW ENGLAND AND NEW AMERICA *and Other Writings of J. Lionel Tayler. With Memoir and Tributes.*

Edited by F. H. Hayward. Williams and Norgate, London. 10s. 6d. net. 9⅞ x 6¼; viii + 335; 1933.

THE STUDY OF INDIVIDUALS (*Science*).

By J. Lionel Tayler. J. W. Sparks, London. 5s. 6d. 8½ x 5½; ix + 241; 1936.

J. Lionel Tayler died in 1930 at age 56 after a career which, if not extraordinary, was at least unusual. He was first trained as a physician but later acquired an interest in theology and eventually became a Unitarian minister. Scientific popularization was his *forte* and for many years public lecturing constituted his major activity. He was a M.R.C.S., L.R.C.P., M. San. I., and London University Extension Lecturer in Biology and Sociology. Although he wrote a number of books he died practically unknown in science or literature. Apparently his lectures were more effective because the friends he acquired from them have formed a committee to honor his memory and by republishing his works hope to arouse interest in his ideas.

The first of the two books mentioned here contains a number of exemplars of Tayler's publications. There are selected passages from some of his writings which will interest primarily persons of a religious frame of mind. The title of the book derives from a philosophical essay in which Tayler praises the cultural traditions of New England. In this volume is also presented an attempt at fiction. It is a sketch of the trials of a man who has temporarily embraced vegetarianism and modifies his daily life to be wholly consistent with that idea. Doctor Tayler clearly was not meant to be a fiction writer.

In the second book is detected the germ of an idea which, if properly developed, would have given Tayler a high ranking among human biologists. In opposition to the methodology of the sociologists Tayler justly believes that the study of social phenomena must be based on a definite knowledge of individual biology. Therefore as a starting point he considers the differences in somatologic constitution. He proceeds to a classification of individual social behavior in terms of anatomical and physiologic personality, then discusses variation in mental characteristics and finally the processes of physical and mental development. For a popular piece of work, this book is neither sufficiently simple nor interesting. As a scientific study it lacks clearness, is too superficial, and demonstrates that the author could not entirely disassociate himself from his religious calling.

THE AMERINDIANS. *From Acuera to Sitting Bull, from Donnacona to Big Bear.*

By Donald M. McNicol. Frederick A. Stokes Co., New York. $2.50. 8 x 5⅛; xix + 341; 1937.

The complete story of the Amerind (or Amerindian, as this author prefers to call him) has never been told, and probably never will be, since except for the astronomic symbols on the Maya megaliths and the undecipherable quipus of South America, he kept no records. The first authentic event in Amerind history is the arrival of the paleface, and from Gaspar Cortereal in the first year of the sixteenth century down to the eviction of the Palatingwas in the twentieth, this history has been a systematic development of exploitation, expropriation, and dishonesty on the part of the white man, with only a

few notable exceptions, such as Bartolomeo las Casas in one continent and William Penn in another.

The tendency of historians of the past has been to dwell on the barbarity of Indian character, to emphasize the tomahawk rather than the pipe of peace. Within recent years the pendulum has swung to the other extreme, with the result that the Indian is now unduly idealized and exalted. As is so often the case, both views are correct in what they affirm and wrong in what they deny. Good and bad people exist among the Indians just as among the whites, and probably in about the same proportion. But there was this important difference between the two races—a difference in population density. At the beginning of European colonization there were probably not over five thousand Indians in Pennsylvania, or a million in all North America. With an abundance of land for all, there was no need for land titles—the Indian had no conception of the private ownership of land. When he sold land, he sold the right to use it and to enjoy the fruits of it insofar as that might be necessary. Even under the land laws obtaining in Europe, in order to clear title to real estate it would have been necessary to obtain a quit claim deed from every Indian in the western hemisphere—and it was easier to shoot them.

The true story of the resulting relations and contacts of the two races can be told impartially by one who knows the Indians as he knows the whites—and there are but few such writers. Mr. McNicol is one of them. He has succeeded in portraying the view point of both sides with equal fidelity, and it is to be hoped that other supplementary works will soon issue from his pen, as the present volume with its index of only five pages, is too small to tell the whole story.

PRIMITIVE INTELLIGENCE AND ENVIRONMENT.

By S. D. Porteus. The Macmillan Co., New York. $3.00. 8½ x 5⅝; viii + 325; 1937.

Many sociologists and psychologists have asserted that the intellectual development of a people is related to the physical environment which it inhabits, in the sense that a harsh environment will have a repressive effect while a pleasant environment will induce a favorable reaction. In order to test the validity of this assertion, Porteus journeyed through the bleakest sections of Australia, the central and north western parts and examined with his maze test 120 aborigines. He then proceeded to the Kalahari desert and there examined 25 Bushmen. Porteus reasoned that if between two primitive peoples that live in a similar harsh environment there is a difference in mental abilities, this difference cannot be attributed to the physical environment but must be considered as due to dissimilarity in innate mental equipment. The results of his survey indicate that the Australian aborigines, whose country is equally as unfavorable if not more so than that of the Bushmen, have distinctly better scores than the Bushmen. A careful examination of his data leads one to doubt the significance of these results. The Australian aborigines had had some schooling at the hands of the whites while the Bushmen had not and the figures presented distinctly show that the Australian subjects who had received no schooling obtained scores inferior to those of the same race who had received instruction. Nevertheless, Porteus concludes, rather tentatively, that in planning ability the Australians are superior while the Bushmen have the advantage in imaginative skill. In the mastery and use of the environment, Porteus believes that the two races are equal.

The presentation of this investigation is not impressive. Only about one-third of the book is dedicated to a description of the results and Porteus devotes a good portion of this section to polemicize with those who have criticized him. The remaining two-thirds of the book contains an account of the author's journey through Australia and South Africa. Porteus justifies this curious arrangement by pleading the need for a thorough description of the habitats of the races examined. However, the account partakes more of the nature of an adventure tale than of an ethnographic or ecologic study.

STUDIES IN THE SCIENCE OF SOCIETY. *Presented to Albert Galloway Keller in Celebration of His Completion of Thirty Years as Professor of the Science of Society in Yale University. Edited by George P. Murdock. Yale University Press, New Haven; Oxford University Press, London.* $6.00. 9 x 6; xxii + 555 + 2 folding charts; 1937.

Everyone of the 26 contributors to this *Festschrift* acquired his doctorate for work done under Professor Keller. Through him, each has been imbued with the ideas of the greatest American sociologist, William Graham Sumner, whose influence is noticeably evident in the majority of the articles.

About one-half of these deal with some aspect of the folkways of contemporary society, the remainder discuss primitive or historical peoples. Included in the former group of articles is an excellent discussion of unemployment benefit by Bakke, and a paper by Davie who presents strong evidence against Burgess's theory of the pattern of urban growth. Among the articles that treat of primitive social behavior patterns there are two, one by Murdoch and the other by Simmons, which deserve special mention. These represent an attempt, somewhat feeble it is true, but nevertheless an attempt, to analyze the concomitance and covariation of social traits by means of statistical methods. These authors have sought to measure for a series of populations the degree of association between given traits, for example, between private property of land and agricultural stage of economy. For this purpose they have utilized Yule's association coefficient, uncritically accepting the results. From a methodological standpoint the procedure is not entirely satisfactory, but a study of these papers should indicate to the general sociologist the need of a finer statistical technique to evaluate generalizations regarding social phenomena.

The main characteristic of all this series of articles is that the discussions are always objective and the conclusions usually based on sufficient data. In comparison with the general run of sociologic literature they manifest a very high standard of scientific quality.

EARLY MEDIEVAL MEDICINE *with Special Reference to France and Chartres. The Hideyo Noguchi Lectures. Publications of the Institute of the History of Medicine, The Johns Hopkins University, Third Series, Volume III. By Loren C. MacKinney. The Johns Hopkins Press, Baltimore.* $2.75. 7¾ x 5¼; 247; 1937.

The three lectures deal respectively with The Dark Age concept and early medieval medicine; Medicine in Merovignian and Carolignian France; and Medical progress at Chartres in the tenth and eleventh centuries. The first of these, a survey of some of the general aspects of medical practice prevailing in Western Europe, develops the view now gaining acceptance by historians of medicine and other fields of human enterprise, that the centuries following the collapse of the Roman Empire have been mislabelled "dark", not because of lack of cultural progress during that era, but because of insufficient evidence so far obtained by modern scholars as to the achievements of the times. The second lecture is concerned with France (approximately the Gaul of Roman times) and the third is restricted to a single locality, the cathedral school at Chartres.

The most frequently quoted sources on the civilization of the Merovignian period are the works of Bishop Gregory (died 593) and other religious writers who placed disease as a consequence of sin and its treatment as a matter of divine intervention. However, the existence of ordinary physicians is evident from the frequency with which they are mentioned, but, not being looked upon with favor by the clergy, detailed records of their enterprises are lacking. During the Carolignian period the study of medicine became a part of curriculum in the monasteries, gradually progressing to an appreciation of classical medical science and intelligent practice of the results of experience.

The author concludes that: "Western medicine, which in the sixth century was well toward the bottom of the ladder, by 1100 had made noteworthy progress. Far from being stagnant and unproductive, the 'dark age' was an era of vigorous activity."

Documentary notes, a series of plates, and an index are provided.

AMERICANS IN PROCESS. *A Study of Our Citizens of Oriental Ancestry.*

By William C. Smith. Introduction by Romanzo Adams. Edwards Brothers, Ann Arbor, Michigan. $3.00. 8 x 5¼; xv + 359; 1937.

This is an interesting discussion of the problems that confront the American born Oriental in Hawaii and in the western states of continental United States. Because of his distinctive physical appearance, the attempts of the American Chinese or Japanese to participate actively in the social and economic life of his native land have been in part successfully opposed by those whites who have delusions of racial superiority or fear of economic competition. In Hawaii, due to the extensive intermingling of many races, the situation has on the whole been much more favorable. But even there an undercurrent of prejudice prevents the Orientals from exchanging freely the status of plantation workers, the prevailing occupation of their fathers, for positions in the clerical or professional fields. So much is this the case that the author advises ameliorating the conditions of the plantations in order that the American Oriental will be satisfied to stay there. In California and other western states the situation of the American Oriental is even worse. It is well known that they are discriminated against in schools and in economic activities, consequently in some a feeling of suspicion and distrust towards whites has developed together with a pessimistic outlook for the future. However, the author believes that here as well as in Hawaii, time will eventually bring about a change in the attitudes of both the Orientals and the whites and will lead to a satisfactory adjustment of their relations. The book is well written and documented and contains useful information regarding the demography of the American Oriental.

RASSENBIOLOGISCHE UNTERSUCHUNGEN *an dem hygienischen Institut der medizinischen Fakultät zu Kanazawa. Numbers 1, 2, 3, and 4. Under the direction of Prof. Dr. Y. Koya.*

Edited by Y. Koya. Kanazawa, Japan. 10⅛ x 7½; No. 1, 4 + 434; No. 2, 3 + 448 + 2 plates; No. 3, 4 + 370; No. 4, 7 + 276 + 3 plates; Nos. 1 and 2, 1936; Nos. 3 and 4, 1937 (paper).

These beautifully printed and edited volumes present the results of a perfectly stupendous amount of biometrical and statistical research in the general field of human biology. The topics in which Prof. Koya and his collaborators are evidently most particularly interested are physical anthropology of the living, human genetics, growth of children, fertility and population problems, and the social aspects of human biology.

Unfortunately for most readers outside of the authors' own country the contents are presented in the Japanese language. For most of the included papers, however, author summaries in German are given. Also in the body of the Japanese text intelligible clues to the meaning of the figures in the numerous tables are generously interspersed.

Space is not available even to list the titles of the contents of these four solid volumes, or much less to give any account of the interesting results achieved. But we can say, and it gives great satisfaction to do so, that no one interested in any aspect of human biology can afford to neglect studying the mass of first-rate quantitative research that is coming out of Prof. Koya's Institute. It makes the impression of being very thoroughly and carefully done, and deals with interesting and significant problems. We shall hope to notice in more detail in these columns future volumes in the series as they appear. In the meantime we wish to congratulate Prof. Koya on the fine work he is directing and producing.

ZUR OSTEOLOGIE DER LAPPEN. *Instituttet for Sammenlignende Kulturforskning Serie B: Skrifter XVIII. Two volumes.*

By K. E. Schreiner. H. Aschehoug and Co. (W. Nygaard), Oslo; Harvard University Press, Cambridge. Kr. 95 for two volumes; Vol. 1, $7.50; Vol. 2, $25.60. 11½ x 9; Vol. 1, 294; Vol. 2, 74 + 278 charts + 101 plates; Vol. 1, 1935, Vol. 2, 1931.

The skulls and partial or complete skeleton remains of 582 Lapps compose the material for this fine anthropological study. The material, now in possession of the University of Oslo, was subjected to the following investigation: Skull measurements and calculation of skull capacities and indices, cranial and facial features described and measured; descriptions of sutures and variations found in them; measurements on 101 children's skulls with calculation of skull capacity and indices; measurements of trunk bones and long bones and various skeletal proportions.

The results of the investigations show, as previous workers have also found, the Lapps to be a mixed race, fitting in between the Mongoloid and Alpine race stocks. It is possible that the Lapps belong to a prehistoric race who before the last ice age lived on the edge of the Altai, whence some wandered to Mongolia, some to Western Europe, while the ancestors of the Lapps went north to Finland. However, as the author points out, it is very difficult to find any very ancient skeletal remains of the Lapps, so it is impossible to come to any definite conclusion about them. Volume 2 of this book contains tables of measurements, skull contours, and photographs. There is a bibliography and an index.

SPORT: Gli Uomini e le Macchine. *Studio Biometrico dello Sport e degli Sportivi.*

By Alfredo Niceforo and Dino Vampa. Societa Editrice del "Foro Italiano", Rome. 9½ x 6¼; xii + 357; 1937 (paper).

A number of years ago Niceforo advanced the idea that the group of human activities classified as sports or athletics would be a fertile field for the application of statistical technique and an additional source of information regarding human variability. The idea is further developed in the first part of this book in which Niceforo outlines the objectives of sport biometrics. In the main, these are to measure (a) the variation in individual productivity, (b) the relation of physical and functional characteristics to athletic ability, (c) the correlation of athletic abilities in several sports, (d) the evolution and progress in the results and in the popularity of each type of sports. The technique to be adopted in such investigations is well illustrated by examples drawn from the records of races, games and other athletic meets.

In the second part of this book, Vampa makes a practical application of Niceforo's ideas principally in the study of 108 amateur athletes of Olympic calibre. A number of interesting facts are brought out; for example, to mention but two, he notes that there is high correlation of individual proficiency in several sports and that the secular changes in speed records can be represented by a logistic type of curve.

This work, based on sound methodologic principles, should serve to stimulate interest in the field of sport as a source of material for the study of human variability. In this country much could be done on this subject since the results of meets and games are easily available and sufficiently accurate.

HISTORY OF THE ARABS.

By Philip K. Hitti. The Macmillan Co., New York. $10.50. 8⅝ x 5¾; xvii + 767; 1937

Professor Hitti, of Princeton University, a native of Mt. Lebanon and educated in the University of Beirut, gives us in this book a most valuable history of the Arabians and the Arabic speaking peoples from the earliest times to the Ottoman conquest of the 16th century. It is based not only on the works of other scholars, but on a first hand and independent study of the original (Arabic) sources. It is accurate in scholarship, lucid and direct in style, appealing to the cultivated layman as well as to the scholar by its touches of humor and its breadth of human sympathy. The author deals with every phase of Arab history, science, civilization and culture, paying particular attention to the various ethnic factors.

What we therefore call "Arab civilization" was Arabian neither in its origins and fundamental structure nor in its principal ethnic aspects. The purely Arabian contribution in it was in the linguistic and to a certain extent in the religious fields. Through-

out the whole period of the caliphate the Syrians, the Persians, the Egyptians and others, as Moslem converts or as Christians and Jews, were the foremost bearers of the torch of enlightenment and learning just as the subjugated Greeks were in their relation to the victorious Romans. The Arab Islamic civilization was at bottom the Hellenized Aramaic and the Iranian civilizations as developed under the ægis of the caliphate and expressed through the medium of the Arabic tongue.

The author has provided an index, genealogical tables, maps, illustrations, and bibliographic annotations. This is a book of the very first rank of importance.

A Black Civilization. *A Social Study of an Australian Tribe.*

By W. Lloyd Warner. Harper and Brothers, New York. $5.00. 8½ x 5½; xviii + 594 + 9 plates; 1937.

In this book Professor Warner gives a detailed description and analysis of the kinship structure, age grading, technology, warfare, magic, medicine, totemism, and mortuary rites of the Murngin, a tropical Australian people. As the tropical Australian tribes have hitherto not been studied as intensively as those of other parts of the continent, this work is a valuable addition to our knowledge of a people of great interest to the anthropologist.

During his first eight or nine months among the Murngin Warner was convinced, as were Spencer and Gillen for the tribes which they had studied, that the people had no knowledge of physiological conception. However, when he inquired directly of certain old men just what the semen did when it entered the uterus of a woman, "they all looked at me with much contempt for my ignorance and informed me that 'that was what made babies.'" His previous failure to elicit this information was due, he suggests, to the greater interest of the savage in ritual and myth, which determine the child's place in the social life of the people, than in the prosaic details of physiology. As far-reaching sociological and anthropological theories have been based on the supposed ignorance of many primitive peoples of physiological conception, he suggests that such speculations be deferred until their factual basis is further investigated.

Back in the Stone Age. *The Natives of Central Australia.*

By Charles Chewings. Angus and Robertson, Sydney. $2.50. 8¾ x 6⅛; xviii + 161 + 23 plates; 1936.

The past year has seen a multiplicity of books of this type, and undoubtedly, many of them have met disapproval because of the monotonous repetition and duplication of material already written. The present volume, however, does not suffer this handicap, because essentially it is the result of long, patient, and acute observation, and not merely the record of happenings of a two-months adventure trip through uncivilized lands.

Dr. Chewings has spent over 50 years in Central Australia, living among the natives and studying their personal and tribal life. He has so dealt with them that he has gained their confidence, and has been able to study at close range their characteristics, habits, beliefs and superstitions. He has also studied the tools and weapons of the different tribes, and has found much interest in observing the effect of the coming of the white man on the life of the native.

The book is written in simple style and language, and is intended for "the man in the streets." The 23 full page plates, which depict the landscape and the peoples of Central Australia, add charm to this enlightening piece of work.

Civilization *as told to Florence Drake.*

By Thomas Wildcat Alford. University of Oklahoma Press, Norman. $2.50. 9 x 6; xiii + 203 + 6 plates; 1936.

Thomas Wildcat Alford, born of parents of the Absentee Shawnee tribe of Indians in 1860, relates the story of his life and of his tribe during the period from which the influence of the white race first began to be felt, up to the present when the so-called civilization of the white man has superceded almost to annihilation the old tribal mode of life.

Early recollections reveal pictures of Shawnee home and tribal life—of child life and parent-child relationship, Indian food, ceremonials, tribal government and many other of the ways of the Indian. Then

comes the picture of the transition from the old cultural life to the ways of the white man. It was not an easy transition and many of the older Shawnees fought bitterly against the intrusion of the white settlers and the ever increasing dominance of their civilization.

Alford, a great grandson of the famed Tecumseh, was trained from early youth for the eventual position of chief of his tribe, but during the course of his education at Hampton Institute he renounced the faith of his fathers and so became ineligible for the desired honor. His life has been devoted, however, to work among his people as an educator, interpreter and leader.

TWENTY-FIFTH ANNIVERSARY STUDIES. *Volume I. Publications of the Philadephia Anthropological Society.*

Edited by D. S. Davidson. University of Pennsylvania Press, Philadelphia; Oxford University Press, London. $2.50. 9 x 6; vi + 235; 1937.

Eighteen articles dealing especially with problems of ethnology and archeology are contained in this volume. The majority of them are short and deal with some particular controversial point. Among the ethnological papers, those deserving special mention include Hallowell's concise but comprehensive description of the frequency of cross-cousin marriages among the aborigines of the Lake Winnipeg region, the influence of this practice on kinship terminology, and the modification of the *mores* due to contact with the Indians and the whites. Another paper of special value is that by Davidson on the question of the relations between Tasmanian and Australian cultures. Davidson lists in a rather convincing manner the principal evidence which leads him to agree with the theory that the Tasmanians originated in Australia. An article by Speck on the medical therapeutic practices of the Catawba Indians and one by Spencer on Fijian dream interpretations would be of great interest to the human biologist if the subject concerned in the articles had been treated in a more complete fashion. With some exceptions this is the general fault of the series of papers contained in this volume, they are well written and documented, but somewhat superficial and not sufficiently informative.

HALF-CAST.

By Cedric Dover. With a Preface on Prejudices by Lancelot Hogben. Martin Secker and Warburg, Ltd., London. 10s. 6d. 8⅜ x 5¼; 324; 1937.

The author of this book, himself a half-caste, feels that the problem of race-intermixture has passed beyond the point of being considered only as isolated, local cases. He believes that it is a general, world-wide problem—and, moreover, that it has been such since the beginnings of civilization and possibly even before that.

In this volume the problem has been considered from a rational point of view which the author hopes may prove suggestive to an intelligent and serious minority. Because he is a scientist Mr. Dover has been able to achieve the almost-impossible: he has succeeded in seeing not only the trees, but the woods as well. However, although the facts presented are impartial, the presentation is delicately flavored with contempt for the irrational smugness and prejudice of the white races in general and various peoples in particular, thus enlivening the discussion.

LE FINNMARKIEN. *Les Origines de la Civilisation dans l'Extrême-Nord de l'Europe. Instituttet for Sammenlignende Kulturforskning Serie B: Skrifter XXXII.*

By Johs. Bøe and A. Nummendal. H. Aschehoug and Co. (W. Nygaard), Oslo; Harvard University Press, Cambridge. N. Kr. 25; $7.80. 12 x 9½; [9] + 263 + 104 plates, 1937 (paper).

During the past year, nearly 60 stations of prehistoric industry have been found and worked in Finmark, the extreme northern tip of Norway. The finds, including very crude tools and weapons, none of which show any evidences of polishing, point very strongly to a unique and substantial early civilization in the extreme north of Europe. The complete lack of fauna from

the stations and the much disputed age of the floral fossils make it very difficult to date the "Finmarkien" either in terms of relative or absolute chronology.

The latter half of the book is made up of 104 plates illustrating 12 station sites and 500 different stone weapons and tools. The bibliography of 188 references does not include all the citations to literature made in the text. The volume contains a brief "Table de Matières", but no index.

GLOBE TROTTING WITH A SURGEON.

By Alexander H. Peacock. Lowman and Hanford Co., Seattle, Washington. $2.50. 8½ x 5¾; 276; 1936.

A new travel story of a trip around the world will always find interested readers, since each globe trotter sees through different eyes and records his views in his own individual style. This is the story of a four months tour made over sea and land, starting from San Francisco and touching Hawaii, Japan, China, the Philippines, Singapore, Ceylon, Bombay, Cairo, Italy, France, England and back across the United States. One is impressed from this account with the great haste with which one—on a tour of this time duration—must continually dash around during those days on land in order to see even a small fraction of the sights at hand. Do not be deceived by the title into believing that medical interests are of any special significance on this trip. The doctor was on vacation. The author's special interest it would seem, was in the art treasures of Italy.

THE DEVELOPMENT OF EUGENIC POLICIES. *Scientific Backgrounds for a New Orientation of Eugenics.*

American Eugenics Society, 50 West 50th Street, New York. 8¼ x 5; 23; 1937 (paper).

CONFERENCES ON THE RELATION OF EUGENICS TO THE FIELDS OF RECREATION—NURSING—EDUCATION AND MEDICINE. *Reported at the Meeting of the American Eugenics Society May 14, 1937.*

American Eugenics Society, 50 West 50th Street, New York. 8¼ x 5; 23; 1937 (paper).

The first of these pamphlets was prepared by the directors of the American Eugenics Society for the members of the conferences held, and to be held, in 1937 and 1938. It deals briefly with such general considerations as the decline in births, the proportion of large families, and the agreement on desirable qualities as well as with the nature of eugenic policies, positive and negative eugenics, etc.

The second pamphlet contains brief reports written by members of the conferences held on the subjects listed.

THE LABOR SUPPLY IN THE UNITED STATES. *Occupational Statistics of the 1930 Census Tabulated by Class of Work and Industry, as Well as by Sex, Race, and Age Groups. Revised June 1937.*

By W. S. Woytinsky. Committee on Social Security, 726 Jackson Place, Washington, D. C. $1.50. 10¾ x 8½; 131; 1937 (paper).

The author has rearranged the occupational statistics of the 1930 census in order to obtain for each class of industry data on the number, age, sex, race(color) and nativity of the persons affected by the provisions of the Social Security Act. This rearrangement necessitated reclassification of the industries and of the classes of work, and separation between independent and salaried workers, between professional persons, managers and other white-collar employees, and between skilled, semi-skilled, unskilled and service workers. The method used is explained in detail and the results presented in 10 tables and 8 charts. On the basis of the 1930 census and assuming certain plausible conditions, it is estimated the 26.3 million persons are affected by the Social Security Act. The conclusions to be drawn from these figures will be presented by the author in a future publication.

THE ETHNOGRAPHY OF THE TANAINA. *Yale University Publications in Anthropology, Number 16.*

By Cornelius Osgood. Yale University Press, New Haven; Oxford University Press, London. $3.00. 9¾ x 7; 229 + 14 plates; 1937 (paper).

The Tananina are a group of Athapaskan-speaking people living on the south coast of Alaska. Their numbers are few, having dwindled from an estimated population of 3000 in 1805 to an estimated 640 in 1932. In this monograph the author has endeavored to reconstruct and "present descriptively the manifest culture of the Tanaina as it would have appeared just previous to historic contact, or in the third quarter of the eighteenth century". This undertaking was rendered particularly difficult because the aboriginal culture has almost entirely disappeared and even the oldest members of the group recall but vaguely earlier tribal traditions and customs. In spite of many difficulties involved in the task, however, the author has accumulated much valuable data on food, dress, shelter, travel, manufactures and implements, war, arts and amusements, social organization, social customs, religion and mythology. Several plates and a bibliography are included.

PRELIMINARY REPORT ON THE EXPEDITION TO SAN AGUSTIN (COLOMBIA). *June–November 1936. Part I. Anthropological Series of the Boston College Graduate School, Vol. II, No. I.*

By Hermann von Walde-Waldegg. Boston College Press, Chestnut Hill, Mass. $1.00. 9¾ x 6⅝; 55 + 2 folding maps + 14 plates; 1937 (paper).

An account of the author's excavations at San Agustin, a site about 300 miles southwest of Bogota, near the southern border of Colombia, where important remains of a prehistoric Indian culture are found. Besides a large number of statues, the author found what he interprets as a calendar stone. Unfortunately, the remains which had been described by Preuss had suffered in the succeeding years from the depredations of treasure hunters. The plaster casts and photographs which Professor von Walde-Waldegg made will form a permanent record of his discoveries.

THE CACTUS EATERS.

By Julian A. Weston. H. F. and G. Witherby, London. 10s. 6d. 8⅛ x 5⅜; 240; 1937.

The cactus eaters are the inhabitants of a wasteland, the Goajira Desert, on the borders of Venezuela and Colombia. The object of the expedition was to take pictures, write a book afterwards, and, if possible, make money via both routes. The result is a very readable and entertaining account of life among the Goajira people. Excellent photographs have been included in large number.

AFRICA'S GOD. *V. Congo and Angola. Anthropological Series of the Boston College Graduate School, Vol. II, No. 2.*

By Joseph J. Williams, S. J. Boston College Press, Chestnut Hill, Mass. $1.00. 9¾ x 6⅝; 137; 1937 (paper).

This book examines the evidence for the belief in a Supreme Being among the Bantu-speaking tribes of Congo and Angola. The author concludes that the belief is held by these tribes but that it is much less precise than in former times. A bibliography of 5 pages is given.

MEDICAL RELIEF ADMINISTRATION. *The Experience in Essex County, Ontario.*

Essex County Medical Economic Research, Windsor, Ontario. 9 x 6; 55; 1937 (paper).

This report describes policies and procedures of medical administrative techniques as carried out on a population on relief. The information given on methods of recording and on control formulas (averages, correlations, probable errors) can be found in numerous elementary statistical text-books.

BULLETIN DER SCHWEIZERISCHEN GESELLSCHAFT FÜR ANTHROPOLOGIE UND ETHNOLOGIE 1936/37. *13. Jahrgang.* [*Bulletin de la Société suisse d'Anthropologie et d'Ethnologie 1936–37. 13me année.*]

Schweizerische Gesellschaft für Anthropologie und Ethnologie, Sempersteig 3, Zürich 1. 9 x 6¼; 22; 1937.

THE COLORADO LABOR MARKET AND ITS RELATION TO UNEMPLOYMENT COMPENSATION. *The University of Colorado Studies, Volume 24, Numbers 3 and 4.*

By Edward R. Livernash. University of Colorado, Boulder. $2.00. 10 x 6⅞; 72; 1937 (paper).

A HISTORICAL SUMMARY OF STATE SERVICES FOR CHILDREN IN OHIO. *U. S. Department of Labor, Children's Bureau, Bureau Publication No. 239.*

Government Printing Office, Washington. 10 cents. 9¼ x 6; v + 38; 1937 (paper).

ZOÖLOGY

OYSTER BIOLOGY AND OYSTER-CULTURE *being the Buckland Lectures for 1935.*

By J. H. Orton. Edward Arnold and Co., London. 5s. 7 x 4⅝; 211; 1937.

This work is extremely timely, for unless the oyster industry can be put on a scientific basis it is likely to suffer and even disappear. The indiscriminate over-fishing of oyster beds has dangerously reduced their production, and the unsupervised introduction of foreign spat has too often been accompanied by that of exotic pests, such as the New England Oyster Drill, *Urosalpinx cinerea*, into Europe and California.

The author discusses in detail the anatomy, growth, alimentation, and reproduction of three species of oyster—one from England, one from Portugal, and one from the eastern United States, but there is much incidental material included concerning oysters from the Pacific coast of America, Asia, and Australia, as well as from France. It was formerly believed that the so called "white-sick" and "black-sick" stages of the European Oyster were indications of sexual differences, but according to this work it is now know that the color is due to the age of the larvae remaining within the parental shell. In the American oyster the eggs are spawned directly into water and there are no color stages. Further, in the European oyster the change of sex occurs immediately after spawning, so that an oyster full of embryos is more likely to be male than not.

The work is not complete, however, for there is no mention of *Pinnotheres*, a crab the female of which lives symbiotically within the mantle cavity of the oyster, the existence of which was known as far back as Pliny's day. Also, the author recommends the transplanting of oysters to relatively fresh water to fatten them for the market, on the ground that the organisms on which it feeds are more plentiful there, but he does not tell us how much of this apparent fattening is merely bloating due to the difference in osmotic pressure, nor does he discuss the deterioration of flavor in fresh water oysters.

There is no index, but there is an interesting glossary of terms used by oyster fishermen.

THE DISTRIBUTION, BREEDING AND FEEDING OF SOME IMPORTANT PLANKTON ORGANISMS OF THE SOUTH-WEST NORTH SEA IN 1934. *Part I. Calanus finmarchicus (Gunn), Sagitta setosa (J. Müller), and Sagitta elegans (Verrill). Fishery Investigations, Series II, Vol. XV. No. 3.*

By R. S. Wimpenny. His Majesty's Stationery Office, London. 3s. net. 10¾ x 7¼; 56 + 2 plates; 1937 (paper).

EXPERIMENTS IN THE BREEDING OF OYSTERS (*Ostrea edulis*) IN TANKS, WITH SPECIAL REFERENCE TO THE FOOD OF THE LARVA AND SPAT. *Fishery Investigations, Series II, Vol. XV, No. 4.*

By H. A. Cole. His Majesty's Stationery Office, London. 2s. net. 10¾ x 7¼; 28 + 4 plates; 1937 (paper).

THE NATION'S SEA-FISH SUPPLY *being the Buckland Lectures for 1936.*

By E. Ford. Edward Arnold and Co., London. 3s. 6d. 7 x 4¾; 112; 1937.

The first article deals with the distribution, breeding, and feeding of *Calanus finmarchicus*, *Sagitta setosa*, and *Sagitta elegans*. Successful broods are dependent on diatom growth and further evidence is offered that the organisms breed in diatom patches which act as nursery grounds.

After several years of unsuccessful experiments, Cole feels that spatfalls of

commercial value can now be obtained from tank-bred oysters. Controlled organic enrichment of the water enables a selective growth of flagellates which practically assures a growth and settlement of oyster larvae. While the spat are capable of digesting the cellulose or hemi-cellulose cell wall of the flagellates, the larvae are unable to do so and must therefore restrict their diet to naked flagellates. A more detailed report is in preparation.

Mr. Ford makes a plea for the proper conservation of the nation's sea resources. He reveals the tremendous wastage that is prevalent and argues that the present intensity of fishing off European waters (applicable anywhere else) which gives little thought to the future will eventually deplete the stock. The Sea-Fishing Industry Act which gives the government power to control unrestricted freedom of fishing was a step forward. "... we stand at a great epoch in our fishing history; we have broken from the past in a tremendous way and now face the future with altered standards of fishing practice, in which state prescription takes the place of personal liberty."

We are always glad to learn of some progress that has been made toward the conservation of the world's natural resources. Somehow fish of the open sea have always been considered immune from extirpation because their aquatic environment makes it difficult to perceive any reduction in numbers, and also since the seas are so vast it does not seem possible that they could be exhausted. Regulating the size of the mesh of the nets is suggested by Ford as the remedy and in the second lecture he discusses this solution. The third lecture is based on the theme: "An ideal sea-fishery is one which is as much concerned about what it leaves behind in the sea, as about what it takes out of it."

BIRDS COLLECTED BY THE CHILDS FRICK EXPEDITION TO ETHIOPIA AND KENYA COLONY. *Part 2. Passeres. Smithsonian Institution, United States National Museum, Bulletin 153. By Herbert Friedmann. Government Printing Office, Washington.* 70 cents. 9¾ x 6¼; xii + 506 + 14 plates; 1937 (paper).

This bulletin is the second publication on the ornithological collections made by the Childs Frick Expedition in Ethiopia and Kenya Colony, and is devoted entirely to the study of the Passerines. The book is based on the collections of birds, nests and eggs, and the observational data made by the late Dr. Edgar A. Mearns. The fact that Dr. Mearns was with the expedition in Africa only 10 months, and that during that time he collected 5200 birds besides a number of nests and eggs, and filled several notebooks with field notes, reveals, in a dramatic fashion, his personal persistence, enthusiasm, and industry.

The work gives a general account of the effect of geographic and climatic factors on bird behavior, and makes some attempt to describe the evolution of birds in Africa. A record of the number of specimens, the sex, the locality and the date of collection, together with the descriptions of color, song, general behavior and distribution is presented. Where natural history data are available, a description of the nesting site, nest, number, size and color of the eggs is also given. The text contains many tables incident to the variation of a species in different localities, and many maps showing the distribution of the different species and sub-species. There are 14 photographic plates showing different birds, nesting sites, and habitats. The table of contents and the index complete the monograph.

A FIRST GUIDE TO SOUTH AFRICAN BIRDS. *By E. Leonard Gill. Maskew Miller, Ltd., Cape Town.* 8s. 6d. 7¾ x 5¼; xv + 223; 1936.

In this *First Guide to South African Birds*, Dr. Gill has met, in some degree, the demand for an introductory work on the country's birds. The study is not to be considered the last word on South African birds, either in the number of birds described, or in the detail with which they are described. The book is intended not for specialists in the field of ornithology,

but for beginners and tourists who may be interested in the South African birds.

The author has followed the general plan of giving for each species the relative size and color of the male and female, the general behavior, the song, the habitat and nesting site, together with the number, size and appearance of the eggs. For the migratory birds, there is also an account of the summer and winter residences, as well as the time of migration. For the naming of the different species, the author has drawn from the work of Mr. W. L. Sclater.

In addition to 20 full page colored plates picturing over 400 species, the book contains a short table of contents, an index of English and Afrikaans names, and an index of scientific (generic) names.

LIFE HISTORIES OF NORTH AMERICAN BIRDS OF PREY. *Order Falconiformes (Part 1). United States National Museum Bulletin 167.*

By Arthur C. Bent. Smithsonian Institution, Washington. 70 cents 9¾ x 6; 407 + 102 pates; 1937 (paper).

This bulletin is the tenth of a series devoted to the life histories of North American birds. The work is developed around the same general plan as has been followed in the previous numbers; namely, giving for each species a general account of the habits, nesting and eggs, followed by a description of the young, the plumages, food, general behavior, range, migration and casual records.

In the general accounts of each species, the author makes use not only of his own broad range of observations from field experience, but also all the important published material, as well as the notes of various amateur observers who have contributed their findings. The bulletin, therefore, represents the extent of present knowledge of our birds of prey (exclusive of the falcons).

The 102 plates present an enormous photographic record of the habits, nests, nesting sites, and appearances of the majority of the species described. The brief table of contents and the extensive bibliography are additional features of this excellent piece of work. We anxiously await the publication of Part 2, which will deal with the Falconidae.

A CATALOGUE OF THE AFRICAN HESPERIIDAE *Indicating the Classification and Nomenclature Adopted in the British Museum.*

By W. H. Evans. British Museum (Natural History), London. 20s. 9 x 5¾; xii + 212 + 30; 1937.

The results of many year's untiring work of collecting and identifying the African Hesperiidae are brought together by General Evans in this catalogue. The author's knowledge of the Indian and Oriental Lepidoptera has been responsible for the detail and accuracy manifest in the preparation of this study.

The work begins with a key to genera which is arranged as explained in the introduction to that section. Then follows a brief diagnosis of each genus, describing points of difference from allied genera, a key to the species of that genus, a synonymic list of species with particulars and descriptions of sub-species and forms, ending with a list of material in the British Museum.

In the 7 colored plates are given figures of all species described for the first time, and of which figures have not hitherto been published. In the 30 plates of uncolored drawings are shown the male genitalia for each species in the Museum. There is an appendix of species described from Africa which have been found to occur elsewhere, or remain undetermined, a bibliography of about 250 references, and an index.

SNAKES ALIVE *and How They Live. Illustrated with Photographs and Including an Illustrated Key for the Identification of the Snakes of the United States.*

By Clifford H. Pope. The Viking Press, New York. $2.50. 8½ x 5½; xii + 238; 1937.

Beginning with his own story of his interest in snakes, his job in the Reptile House of the Bronx Zoological Park, and his adventures as a snake collector in China, the author goes on to a more de-

tailed discussion of the various kinds of snakes, their usefulness, their reproductive habits, their possible size and age, and the popular beliefs that have sprung up about them. On the whole it is a well written account and makes interesting reading. However, here and there it seems as if the author suddenly becomes aware of a moron reader and adds a sentence or two of irritatingly childish explanation. The volume is illustrated by splendid photographs and occasional drawings. There is an appendix containing a simplified key for the identification of the genera and species found in the United States, and a detailed index.

STUDIES IN THE LIFE HISTORY OF THE SONG SPARROW. I. *Transactions of the Linnaean Society of New York, Volume IV.*

By Margaret M. Nice. Linnaean Society of New York, c/o American Museum of Natural History, New York. $1.50. 9 x 6⅛; vi + 247; 1937.

To find a study in natural history that shows more patience and keen observation in the field than this, and more accuracy and selectivity in organization of material, would indeed be a difficult task. Over a period of eight years the author has made this intensive study of the habits and social behavior of the Song Sparrow, in an attempt to reveal some of the factors influencing its population, not only from season to season of the same year, but from year to year.

Each of the 20 chapters of the study is briefly summarized, and the salient points of the entire study are brought together in a general summary. The bibliography of more than 200 references shows something of the author's thorough acquaintance with the subject. The table of contents, appendices I to V, and the indices (1) of subjects, and (2) of species, are features incident to the completeness of the monograph.

POST-MORTEM EXAMINATIONS OF WILD BIRDS AND MAMMALS. *U. S. Department of Agriculture, Miscellaneous Publication No. 270.*

By J. E. Shillinger. Government Printing Office, Washington. 5 cents. 9¼ x 6; 16; 1937 (paper).

As field workers engaged in activities involving wild life frequently come across sick and dead animals of various species, this publication has been written to assist them in noting abnormal conditions and in helping to eradicate them by reporting the facts observed to wild life disease-control specialists.

The author discusses the various causes of death and symptoms of diseases. With the aid of photographs he tells how to perform autopsies on mammals and birds, going into the external factors involved, the dissection, the condition of the internal organs, and the examination of the head. A separate paragraph is devoted to each of the six most common causes of wild life losses; space is given to suggestions for preserving specimens for study; directions for shipping them to the laboratory; and hints on safeguarding investigators against infection.

TERMITE CITY.

By Alfred E. Emerson and Eleanor Fish. Foreword by William Beebe. Illustrations by Keith Ward. Rand McNally and Co., New York. $1.50. 8½ x 6½; 127; 1937.

This is the story of the highly socialized life of a termite colony and the routine of its various inhabitants. There is the bloated queen and her mate who spend their long lives of from twenty to fifty years in a single cell of the termitarium, producing tiny workers and slightly larger soldiers to build and to protect their nest, and other queens and kings who fly away to start new colonies.

It is a story that should captivate readers of all ages, beautifully and simply written, yet without a trace of the artificial simplicity so often found in children's books. As Beebe so aptly remarks in the foreword, "It is wholly free from such absurdities as 'Tommy Termite' and 'Nellie Nymph'."

A glossary supplements the text describing in more detail, but with the same simple clarity, many of the topics mentioned only briefly in the story. There is also a complete index.

FLIGHT SPEED OF BIRDS. *U. S. Department of Agriculture, Circular No. 428.*
By May Thacher Cooke. Government Printing Office, Washington. 5 cents. 9¼ x 6; 14; 1937 (paper).

As the possible flight speed of birds, usually computed on estimations alone, has always been greatly exaggerated, the author sets about to give some actual facts on the subject. In this paper she discusses individual speed variation among the same species, and the effect of size and shape of wings, the weight of the bird, and the influence of the wind on speed. In conclusion she gives a table of speeds showing the miles per hour flown by over a hundred different species as timed by airplane, automobile, ship, etc. A few running speeds are also included. The source of all the material, excellently summarized in the table, is given in a complete bibliography.

NEMATODES PARASITIC IN ANIMALS.
By Geoffrey Lapage. Methuen and Co., London. 4s. 6d. net. 6¾ x 4¼; x + 172; 1937.

THE NEMATODE, ORNITHOSTRONGYLUS QUADRIRADIATUS, A PARASITE OF THE DOMESTICATED PIGEON. *U. S. Department of Agriculture. Technical Bulletin No. 569.*
By Eugenia Cuvillier. Government Printing Office, Washington. 10 cents. 9¼ x 5⅞; 36; 1937 (paper).

The book by Lapage is primarily a topical summary of the works of many workers, with conclusions drawn by the author. Nematode physiology is stressed, along with the need for further study in this field by experimental biologists so that better control measures may be determined.

In the Cuvillier bulletin is presented the study of a serious infection of pigeon flocks. Control birds were used to determine the relations between host and parasite, and the effect of external environment on eggs and larvae, in order to show methods of control valuable to pigeon raisers.

RAPPORT ATLANTIQUE 1934–1936 (*Travaux du Comité du Plateau Continental Atlantique*). *Rapports et Procès-Verbaux des Réunions, Volume CIV.*
Conseil Permanent International Pour l'Exploration de la Mer. Published with the aid of Ed. Le Danois and Rafaël De Buen. Andre. Fred. Host and Fils, Copenhagen. Kr. 2.00. 10½ x 8¼; 37; 1937 (paper).

The Atlantic Slope Committee was created for the purpose of establishing a liaison between the *Conseil International pour l'Exploration de la Mer* and the *Conseil International de Recherches des Pêcheries de l'Amerique du Nord*. The object of the Committee is to conduct research on the problems common to both the American and the European coasts of the Atlantic Ocean. This report describes in some detail the studies that have been made in 1935 and 1936 and that have been presented at the annual meeting of the International Council.

DIE LACHSARTIGEN (*Salmonidae*). *I. Teil. Handbuch der Binnenfischerei Mitteleuropas, Band III, Lieferung 5.*
By E. Neresheimer. E. Schweizerbart'sche Verlagsbuchhandlung, Stuttgart. In Germany: 25 marks; Outside of Germany: 18.75 marks. 10½ x 7½; vi + 152; 1937 (paper).

This treatise on the various species of salmon found in Central Europe, deals with the anatomy, development, mode of life, mating, breeding grounds, and interrelationships between species. It is profusely and well illustrated and though primarily intended for the research worker or fishery official, can be used to advantage by the layman. There is a bibliography and an index.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society. Volume XXII, Part 2, Numbers 7–13. Containing following articles: The Templeton Crocker Expedition VI. Oxystomatous and Dromiaceous Crabs from the Gulf of California and the West Coast of Lower California*, by Jocelyn Crane; *VII. Caridean Decapod Crustacea from the Gulf of*

California and the West Coast of Lower California, by Fenner A. Chace, Jr.; *VIII. Polychaetous Annelids from the West Coast of Lower California, the Gulf of California and Clarion Island*, by Aaron L. Treadwell; *IX. Holothurians from the Gulf of California, the West Coast of Lower California and Clarion Island*, by Elisabeth Deichmann; *Notes on the Cestodes of North American Sparrows*, by H. W. Stunkard and John J. Milford; *Further Studies on the Susceptibility and Acquired Immunity of Marine Fishes to Epibdella melleni, a Monogenetic Trematode*, by Ross F. Nigrelli; *Further Notes on Certain Birds of Paradise*, by Lee S. Crandall.

New York Zoological Society, Zoological Park, New York. $1.40. 10½ x 7; 97–195 + 5 plates; 1937 (paper).

A List of the Beetles of San Diego County, California. *Occasional Papers, San Diego Society of Natural History, Number 2.*

By Ian Moore. Society of Natural History, San Diego, Calif. 8¼ x 5¼; 109; 1937 (paper).

A Further Report on Birds from Sonora, Mexico, with Descriptions of Two New Races. *Transactions of the San Diego Society of Natural History, Volume 8, No. 23.*

By A. J. van Rossem and The Marquess Hachisuka. Society of Natural History, San Diego, Calif. 10¼ x 7; 16, 1937 (paper).

Thelazia Californiensis, a Nematode Eye Worm of Dog and Man, with a Review of the Thelazias of Domestic Animals. *University of California Publications in Zoology, Volume 41, Number 17.*

By Charles A. Kofoid, Owen L. Williams, and N. C. Veale. University of California Press, Berkeley. 25 cents. 10¼ x 6¾; 9; 1937 (paper).

The Action of Specific Immune Serum on Infections of Trypanosoma hippicum Darling in the Rat. *University of California Publications in Zoology, Volume 41, Number 18.*

By Horace F. Sharrocks. University of California Press, Berkeley. 25 cents. 10¼ x 6¾; 14; 1937 (paper).

The Neuromotor System of Nyctotherus hylae. *University of California Publications in Zoology, Volume 41, Number 19.*

By Lauren E. Rosenberg. University of California Press, Berkeley. 35 cents. 10¼ x 6¾; 27; 1937 (paper).

Gene and Character. IV-VIII. *University of California Publication in Zoology, Volume 41, Numbers 20–24.* Containing the following articles: *IV: Further Data on the Development of Wing Mutants in Drosophila* and *V: Further Data on the vg Dominigenes in Drosophila melanogaster*, by Richard Goldschmidt; *VI: Dominigenes and vg Allelomorphs*, by Richard Goldschmidt and Elizabeth Höner; *VII: The "Nonhereditary" kn Effect in Drosophila* and *VIII: A Selection Experiment with Dominigenes*, by Richard Goldschmidt.

University of California Press, Berkeley. 75 cents. 10¼ x 6¾; 65; 1937 (paper).

BOTANY

The Garden of Gourds.

By L. H. Bailey. Macmillan Company, New York. $2.50. 9¼ x 6; 134; 1937.

Gourds are so simple to cultivate that this book is not a horticultural guide but rather a series of descriptions of the many different groups. The gourd family is a large one including all the melons, cucumbers, pumpkins and squashes. The gourd of North America is defined as "a hard-shelled durable fruit grown for ornament, utensils and general interest." Their present popularity is indicated by the formation of the International Gourd Society in California. The organization publishes a semi-annual bulletin.

The author's enthusiasm over the diversities and beauties of the gourds is highly infectious. The reader immediately wants to try some in his own garden. Dr. Bailey says that he "has grown gourds in each year since his youth." He has, therefore, collected an enormous amount of information on the subject. His pen and ink drawings add greatly to the interest and artistic value of the book.

The historical ancestry of the gourd family is traced back to pre-Linnaean time. This early period is graphically shown in a set of reproduced illustrations some of which go back to Lobel in the sixteenth century.

There is a colored frontispiece and a comprehensive index.

STUDIES ON THE DEVELOPMENT OF CONIFERS IN RAW HUMUS. III. *The Growth of Scots Pine (Pinus silvestris L.) Seedlings in Pot Cultures of Different Soils Under Varied Radiation Intensities. Meedelanden Från Statens Skogsförsöksanstate, Häfte 29, Nr. 7* *By P. R. Gaast. Reprinted for the Harvard Forest, Petersham, Mass.* Free. 6⅞ x 9¾; 587–682; 1937 (paper).

This represents a physiological study of the growth of pine seedlings. Scots pine and white pine (*Pinus strobus* L.) were grown under controlled conditions. The author believes that the limits of experimentation by varying a single factor have practically been reached. His work, therefore, involves the simultaneous variation of two factors: mineral nutrition and radiation. The paper contains not only detailed descriptions of the experiments performed and their results, but also statistical analysis of the results. In order to determine the effects upon the seedlings of varying two factors, other factors such as seed-weight must be carefully considered. By applying mathematical formulae the author has determined relationships, some logarithmic, some proportional, etc., which enable him to separate these various effects.

Tables, graphs, a list of references, and a summary in Swedish (some of the experiments were done at the Swedish Institute for Experimental Forestry) are included.

WÖRTERBUCH DER DEUTSCHEN PFLANZENNAMEN. *Lieferung 1, Abelia—Agrimonia. By Heinrich Marzell. Unter mitwirkung von Wilhelm Wissmann. S. Hirzel, Leipzig.* RM. 5. 11¼ x 8¼; x + 144; 1937 (paper).

This ambitious work is an attempt to list approximately eighty thousand vernacular German names of plants under the corresponding generic terms alphabetically arranged. It is being issued serially and the first installment runs from Abelia to Agrimonia.

Although German scientists have carried their researches to the ends of the earth they have contributed little to this work, for the scientist uses the more exact terminology of his science and can get along very well without vernacular names. The only plants having German vernacular names are those growing in countries where this language is spoken, such as in central Europe, those growing in regions settled by German immigrants, such as Lancaster County, Pennsylvania, or those which have a pharmaceutical or commercial value, so that their products are imported into Germany. The geographic ranges of the plants covered are therefore very variable and inexact: this however, is not a disadvantage when the purpose for which the work was undertaken is considered. It is a dictionary, and the metes and bounds of dictionary definitions must be essentially linguistic rather than geographic. The illustrations are well drawn and the type is good. Altogether, the work is likely to prove useful.

PHYTOHORMONES.

By F. W. Went and Kenneth V. Thimann. The Macmillan Co., New York. $4.00. 8⅜ x 5½; xi + 294; 1937.

As our knowledge in this field has progressed the terms "growth substance" and "growth hormone" have been replaced by the more specific term "auxins," applying to a group of substances affecting cell elongation. In this volume the authors deal only with the hormones of the higher plants. They attempt to show how "correlation proper, organ formation, tropisms and normal growth have . . . . been unified into a complete picture of hormone activity as we now know it."

The book includes introductory background, detailed descriptions of experimental technique, results of experiments, and new theories. Some of this material has not previously been published. The chief fault of which the authors can be accused is over-enthusiasm. As often happens in a fairly new and rapidly developing field, there is a tendency to attribute practically the entire development of the plant to hormone activity.

Time usually suffices to produce the proper perspective. But of course there is always the possibility that in this case the proper perspective has already been achieved. We can but wait and see. In the meantime, however, the authors have given us an excellent summary of the present situation.

STUDIES ON WHEAT GROWN UNDER CONSTANT CONDITIONS: *a Monograph on Growth.* *By H. L. van de Sande-Bakhuyzen. With contributions by Elizabeth P. Griffing and Carl L. Alsberg. Food Research Institute, Stanford University, Cal.* $4.00. 8½ x 5¾; xvi + 400; 1937.

The object of this study was to determine the "standard wheat plant" or the inherent, intrinsic rate of growth of the wheat plant. With all environmental conditions controlled leaf areas were measured, growth curves plotted, dry weight, carbon and nitrogen content determined, etc. The hope was that the results of the study would facilitate more accurate forecasting of crop yields from the weather, "the weather" in this case being considered as a single variable. The observations and analyses are extremely detailed and all sorts of relationships and correlations have been determined. The Directors of the Food Research Institute, the instigators of this investigation, do not consider this piece of work as a complete solution to the problem; rather it is to be considered as a beginning for future work.

The text is illustrated with graphs and tables, and an index and detailed bibliography are included.

HARDY CALIFORNIANS. *By Lester Rowntree. The Macmillan Company, New York.* $3.50. 8½ x 5½; xiv + 255; 1936.

This book introduces its readers to many of the hardy but less familiar native wild plants of California. The author is a plant enthusiast who has literally dug into all niches and corners of the state to observe and study the several hundred plant species so colorfully described here. For the garden maker who would like to learn the secrets attending successful culture of wild plants in a new environment, careful notes have been made on exposure, type of soil and other requirements. The author has found that many California plants generally considered tender are, in reality, quite hardy if planted in the proper environment. The book is illustrated with about seventy photographs taken by the author, of close-ups of plants growing in their natural habitats. The volume is indexed.

BACTERIOLOGY. *A Text-Book of Microörganisms. Third Edition.* *By Fred W. Tanner. John Wiley and Sons, New York; Chapman and Hall, London.* $3.50. 9 x 5⅞; xiii + 510; 1937.

The first edition of this text received notice in Vol. 4, No. 3 of the Quarterly. The present edition is, in essence, an exact duplicate of the earlier one with few minor differences. The author has made a few additions, alterations, and omissions in his arrangement and discussion of material, and has included about a dozen new illustrations in the text. He has also brought the bacteriological literature up to date.

The text is developed around the fundamentals of bacteriology, and so is intended more for the beginner than for the advanced or special student in microbiology. It contains a table of contents, an appendix of bacteriological literature, a glossary, a topical outline for lectures in microbiology, and a lengthy index.

LA VIE DE LA CELLULE VEGETALE. *L'Enveloppe de la Matière Vivante.* *By Raoul Combes. Armand Colin, Paris.* 13 francs. 6¾ x 4½; 216; 1937 (paper).

This is the third volume of the author's intensive study of the plant cell; the first two being devoted to *La matière vivante*, and *Les enclaves de la matière vivante*. In the present volume, M. Combes discusses (1) the morphological, physical and chemical structure, together with the general physiology, of the cell membrane; and (2)

the morphological and chemical structure, together with the general physiology, of the bodies that collect either in the cell membrane or in the intercellular spaces.

The work is based largely on the author's own experimental studies, but it contains some 250 citations to literature relative to the subject. The book contains a number of interesting drawings and diagrams, a short table of contents and a bibliographic index.

FLORAL MORPHOLOGY. *A New Outlook with special reference to the Interpretation of the Gynæceum. Volume I.*

By E. R. Saunders. W. Heffer and Sons, Cambridge. 3s. 6d. 7¼ x 4¾; viii + 132; 1937.

Unlike systematic botany which treats the subject of floral morphology almost entirely on a basis of the external characteristics of the plant, this book considers the external features in relation to the vascular system of the plant. This new approach has led to the solution of many morphological problems and, in other cases, to the simplification of previously accepted explanations. The first part of the book consists of an exposition of the general principles which are regarded as underlying floral arrangement. The remainder, and larger portion, discusses thirty-nine Families considered from the new point of view. The author hopes to publish a second volume which will deal similarly with other families.

GENERAL AND ECONOMIC BOTANY.

By Ernest E. Stanford. D. Appleton-Century Co., New York. $4.00. 8¾ x 5⅞; xxix + 675; 1937.

This book has been designed as a text for elementary college botany. It differs from a large number of other introductory texts in that it emphasizes the economic rather than the purely scientific aspect of plants. This being so, it is natural that the main approach should stress morphology rather than physiology since "the principal significances and utilities of plants derive more obviously from form and composition than from function." As for content, the usual first-course-in-botany material is covered, although perhaps more adequately from the point of view of general information than as a foundation for advanced work in the field. Interesting drawings and photographs and an index are included.

PLANTS USEFUL TO MAN. *Second Edition.*

By Wilfred W. Robbins and Francis Ramaley. P. Blakiston's Son and Co., Philadelphia. $3.50. 8½ x 5⅝; ix + 422; 1937.

The three principal improvements of this edition over the first (reviewed in Volume 9, page 113 of this QUARTERLY) are (1) additions to, or modifications in the accounts of alfalfa, flax, figs, potato and various ornamental plants; (2) a rearrangement of parts of the chapter on sources of cultivated plants in order to bring out more clearly the original native homes of plants and to emphasize the early centers of agriculture; and (3) replacement of some of the figures.

ON THE PLACE OF ONTOGENY IN FLORAL ENQUIRY. *Publications of the Hartley Botanical Laboratories, No. 17.*

By John McL. Thompson. University Press of Liverpool, Liverpool. 3s. 6d. 12 x 8½; 20; 1937 (paper).

This is a technical discussion based on an article by Dr. Kozo-Poljanski which in its turn was based chiefly on the work of Dr. H. H. Thomas and Professor Thompson. The author has written this paper to clarify "some of the conceptions to which special reference has been made" and for this reason has considered it appropriate to examine the basis of Dr. Kozo-Poljanski's objections.

SILVA FENNICA 39. *Metsänhoitajien Jatkokurssit.* [*Der Fortbildungskursus der Forstmeister 1935.*]

Society of Forestry in Suomi, Helsinki. 9¾ x 6½; 310; 1937 (paper).

ACTA FORESTALIA FENNICA 44. *Publications of the Society of Forestry in Suomi.*
Helsinki. 9⅝ x 6½; 468; 1937 (paper).

ETUDES DE DEVÉLOPPEMENT FLORISTIQUE EN LAURENTIE. *Contributions du Laboratoire de Botanique de l'Université de Montréal No. 27.*
By Frère Cléonique-Joseph. Institut Botanique, Université de Montréal, Montréal. (Obtainable also from Henry G. Fiedler, New York and T. Oswald Weigel, Leipzig). $1.50. 9 x 6; 246; 1936 (paper).

MORPHOLOGY

STRUCTURE OF THE VERTEBRATES. *Revised Edition.*
By Malcolm E. Little. Farrar and Rinehart, New York. $3.00. 8¼ x 5½; x + 488; 1937.

Presented by a professor of education who is thoroughly versed in comparative morphology, the material of this college text is well organized for both teacher and student. It is capable of fairly close correlation with a laboratory course. The book omits much of the detail in histology, embryological development, and anatomy "in order to help the student gain a better understanding of fundamental relationships" in the evolution of the structure of man. Accordingly each system considered leads in this direction, and the final chapters are devoted to evolution. There are many good drawings, a complete index, and a glossary which includes Latin or Greek derivations.

VERGLEICHEND-ANATOMISCHE, EXPERIMENTELLE UND EMBRYOLOGISCHE UNTERSUCHUNGEN ÜBER DAS NERVENSYSTEM UND DIE SINNESORGANE DER RHYNCHOTEN. *Zoologica, Heft 93, Lieferung 1 and 2.*
By Otto Pflugfelder. E. Schweizerbart'sche Verlagsbuchhandlung, Stuttgart. In Germany: 74 marks; Outside of Germany: 55.50 marks. 12½ x 9¾; vi + 102 + 25 plates; 1936 (paper).

This is a thorough piece of research on the nervous system of the Rhynchota. After a description of the material used, the author describes the external morphology of the Heteroptera and Hymenoptera. There follows a long histological discussion of the brain of various species of this order of insects. The last section deals with experimental work involving removal or destruction of sense organs; the effect of this on the brain was unnoticeable. There is a bibliography and index, also many excellent plates and drawings.

LABORATORY STUDIES IN COMPARATIVE ANATOMY.
By W. C. Senning. McGraw-Hill Book Company, New York. $1.75. 9 x 6; ix + 188; 1937.

An excellent laboratory manual for the teaching of elementary comparative anatomy. Very little is done with type forms, the main emphasis being on a study by systems of organs. The shark, *Necturus*, and the cat are the animals used, student dissections being supplemented by frequent use of demonstration specimens.

GRUNDRISS DER ENTWICKLUNG DES MENSCHEN. *Zweite, neu bearbeitete Auflage.*
By A. Fischel. Julius Springer, Berlin. RM. 12.60. 9¾ x 6½; v + 143; 1937.

The first edition of this work was reviewed in Volume 7, Number 2 of the Q.R.B. The second edition follows the first in outline, with here and there reference to more recent work in human embryology. This work still suffers from the lack of a bibliography.

PHYSIOLOGY AND PATHOLOGY

MECANISMO PROBABLE DE LA CANCERIZACIÓN. (*Ensayo Patogénico*). *Two Volumes.*
By Americo Garibaldi. Facultad de Ciencias Médicas, Universidad Mayor de San Marcos, Lima. 10 x 7; Vol. 1, xxxii + 281; Vol. 2, 327; 1936 (paper).

The author presents here a theory to end all theories regarding the mechanism of cancer growth. His theory results from the attempted integration of all the basic

observations which have led to the numerous hypotheses relative to the mode of development of cancer. He reviews critically the assumptions underlying a number of them, over 30 in fact, and concludes that all are in some degree inadequate because they do not interpret the facts in terms of cellular physio-pathology. Briefly put, his own theory is based on the assumption that a cell that will develop cancer is characterized by unstable metabolism and altered surface tension. This would explain the hypercholesterinemia and hyperglycemia so often found and it would also explain the increased size of the nucleus relative to the cytoplasm, the only real morphologic change to be observed in a cancerous growth. Further considerations on this point lead him to state that a cancerous cell is a cell with a tendency towards nuclear and cytoplasmatic hypertrophy, but which reacts to this tendency through the process of proliferation. To explain cancer formation he contends that it represents an activity analogous to the reaction of normal tissue to infection. With this in mind he proceeds to the final formulation of his theory: Cancer is a defense mechanism against the metabolic insufficiency and disturbance of surface tension of the cell.

The author's *tour de force* is admirable even though the excessive rationalization based on insecure grounds is not so convincing. He deserves to be read. The bibliography is extensive but the citations have not been given in complete form.

Some Quantitative Aspects of the Biological Action of X and γ Rays. *Medical Research Council, Special Report Series, No. 223.*

By C. M. Scott. His Majesty's Stationery Office, London. 1s. 6d. net. 9⅛ x 6; 99; 1937 (paper).

It is assumed that radiation which passes directly and unchanged through a tissue has no biological effect, the changes, if any, which occur being produced by fluorescent absorption and by the scattering of radiation with consequent change in wave length. In order to measure the dose of radiation administered, the *r* unit, based upon the absorption of radiant energy, in air, is in common use today. Unfortunately, this does not take into account the great variation in absorption according to size, shape, substance, and depth of tissue being treated. Due to this difficulty of measurement of dose, even when an attempt at measurement is made at all, and to great variation in techniques used, many workers have obtained directly conflicting results. In spite of the difficulties, Scott has done an admirable piece of work in summerizing and comparing the major findings of workers in this field. In most instances, the therapeutic value of radiation is dependent upon its differential effect on normal and on abnormal or diseased tissue. It is therefore important to discover how various tissues differ in this respect and what artificial factors may be introduced to modify their sensitivity to radiation. Scott tells what evidence there is in regard to this, adds a considerable amount of experimental evidence of his own, and has done much to clarify the issues. As the brochure is short and clearly written, those in other fields of biology should find it interesting reading. The bibliography is up to date and quite extensive.

Les Épidémies et l'Histoire.

By Albert Colnat. Éditions Hippocrate, Paris. 30 francs. 9⅝ x 6⅛; 191; 1937 (paper).

This monograph introduces what might be called an epidemiological view of history. Man is and always has been a victim of disease, in the past as now. This has affected his mode of living and his activities. Consequently the sequence of past events which we call history has been affected to a greater or less degree by the presence or absence of disease. In this respect, the epidemic diseases whose actions are manifested simultaneously on a mass of people are of particular importance in determining certain changes in the course of events. The author recalls how greatly the Pest at Athens contributed to the downfall of that city during the Peloponnesian Wars. He notes that the series of epidemics known as the

Antonine Pest and the Justinian Pest probably contributed more to the disruption of the Roman Empire than did the barbarians. The influence of epidemics on the outcome of wars is discussed in relation to the Crusades, to the Hundred Years War, and down to the World War, in which typhus was a factor in blocking the conquest of Serbia by the Austro-Hungarians.

The idea outlined here is far from original but it has still to be developed in an adequate manner. The author's attempt is a step forward although he has not really succeeded in investigating with any degree of thoroughness the complex ramifications of the relation between disease epidemics and the course of human events.

ENDOCRINOLOGIE CLINIQUE, THÉRAPEUTIQUE ET EXPÉRIMENTALE.

By P. Sainton, H. Simonnet and L. Brouha. Masson et Cie, Paris. 130 francs. 10 x 6¾; viii + 834; 1937 (paper).

This treatise covers in an authoritative manner the present state of knowledge regarding the normal and pathologic structure and function of the endocrine glands. The first chapter describes in sufficient detail the development of this branch of medicine and the methods of research. This chapter is especially well written and contains important information, usually omitted in the majority of textbooks. Following this, there are chapters on the thyroid, the parathyroids, the adrenals, the hypophysis, the pineal, the gonads, the thymus, and the pancreas. For each of these glands are specified the particulars of anatomy, physiology, biochemistry, experimental work, clinical symptoms of hyper- or hypo-function, and therapy. In separate chapters are discussed the function of the endocrines in pregnancy and in the various forms of intersexuality. The authors also present a critical review of the studies which have demonstrated or presumed to demonstrate hormones in the digestive tract, salivary glands, and other organs of the body. Throughout this book one notes that the authors have evaluated with a great deal of objectivity the discoveries and observations reported, and the reader is always made aware of the relative validity of the conclusions reached. Only one major criticism can be made: there is no bibliography.

VISUAL PERCEPTION.

By M. D. Vernon. The University Press, Cambridge; The Macmillan Co., New York. $4.50. 8½ x 5½; xi + 247; 1937.

This treatise is a "halting and incomplete" explanation of (1) the stages in the development of perceptions; (2) the relation of these perceptions to affective individual states; (3) the structure of the perceptual field; and (4) the development of the perceptual content during childhood.

The volume is written in a very technical manner, and emphasizes, in minute detail, the experimental procedures and observations carried out. Although the author has drawn extensively from Gestalt psychology, she is critical of the use of the theory as a general explanation for perceptions. The author's purpose has been to "dig the ground, clear away the debris, and perhaps prepare the foundation" for the structural explanation of perceptions in terms of psychology and physiology. The book is a careful achievement of this purpose.

The volume contains a bibliography of nearly 400 references, a detailed table of contents, an appendix describing and illustrating the tachistoscope, an index of authors and an index of topics.

CHILDBIRTH: *Yesterday and Today. The Story of Childbirth Through the Ages, to the Present.*

By A. J. Rongy. Emerson Books, Inc., New York. $2.00. 5⅛ x 7⅞; 192; 1937.

SAFELY THROUGH CHILDBIRTH. *A Guide Book for the Expectant Mother.*

By A. J. Rongy. Emerson Books, Inc., New York. $2.00. 5⅛ x 7⅞; 192; 1937.

These two volumes, each complete in itself, together form a connected story of childbirth from ancient times to the present day. From such a comparative study

one gains an appreciation of the advantages held by the prospective mother of today over her sister of past ages when in place of the scientific knowledge of the present, childbirth was attended by ignorance and superstitions.

The second of these books contains a short but up-to-date account of the development of the embryo and foetus with accompanying changes in the mother's condition, descriptions of the processes of labor and normal and operative births. There are chapters on pre-natal and post-natal care and spontaneous abortion. The book is sensible and practical and a good one to be placed in the hands of prospective mothers. Each volume has an index and the second is equipped with a glossary, but bibliographies are lacking.

SOME FUNDAMENTAL ASPECTS OF THE CANCER PROBLEM. *Symposium Sponsored by the Section on Medical Sciences of the American Association for the Advancement of Science. Atlantic City, New Jersey, December 29, 1936—January 1, 1937.*

Edited by Henry Baldwin Ward. The Science Press, New York. $2.50 (cloth); $2.00 (paper). 10¼ x 7⅛; 248; 1937.

The purpose of this symposium was to give a survey of the latest work done by leading American investigators on the many sides of this problem. The thirty-one papers presented at Atlantic City are all included in this volume in their original form and with their original conclusions. Certain of these papers, such as *Genetics of Cancer and Its Localization* by Maud Slye, excited some very lively discussion at the meeting which unfortunately was not recorded at the time and so could not be included here. The symposium was divided into several sessions according to subject matter, seven papers being given on heredity and constitutional factors, nine papers on induction, stimulation and inhibition of tumorous growths, five papers on metabolism of cancerous tissue, and seven papers on the effect of radiation. Louis L. Dublin gave a *Statistical Analysis of Mortality from Cancer.* C. C. Little discussed the *Social Significance of Cancer,* and Walter Schiller reviewed *Changes and Modifications in the Conception of Carcinoma* in a final session for the general discussion of the cancer problem.

TEXTBOOK OF GENERAL PHYSIOLOGY. *By T. Cunliffe Barnes. P. Blakiston's Son and Co., Philadelphia.* $4.50. 9 x 6; xxii + 554; 1937.

In the past we have found it necessary to criticize text books on this subject on the score of being dogmatic and of giving neither proof nor references for their statements. We are, therefore, glad to say that Dr. Barnes prefers to cite the evidence rather than to state "laws", and that he gives numerous references to every subject dealt with, his bibliography listing over 1,600 papers. Naturally, no student is going to look up all these papers, but this presentation will encourage him to go more deeply into any phase of the subject that especially catches his interest.

The distribution of space and the subject matter covered is on the whole quite usual. Naturally, more than usual attention is given to the properties of water, since this is a field in which the author has done much work. This is rationalized on the ground that water is the most universally vital compound for living organisms.

The text is well written and there are many illustrations and diagrams. The inclusion of a number of semi-comic cartoons is a unique feature of the book.

AIDS TO PHYSIOLOGY. *Second Edition. By Henry Dryerre. William Wood and Co., Baltimore.* $1.25. 6¼ x 3¾; vii + 295; 1937.

This book is clearly written and well compiled for its purpose, but we are not so sure that, its purpose being fulfilled, the results would be desirable. It is a sketchy and abbreviated outline of the subject, with the emphasis strongly placed on definitions of a great number of technical terms which are printed in bold face type. Its "aid" to students, so far as we can see, would be mainly in cramming for exami-

nations and this is why we are a little dubious as to its value. In our experience, one of the greatest benefits of an outline of this kind is derived from the actual making of it by the student himself, as this forces him to give close attention to the subject and impresses it upon his memory. On the other hand, workers in other fields might well find it convenient as an extended dictionary to refresh their memory when they have to read a physiological paper.

TRAUMA AND DISEASE.

Edited by Leopold Brahdy and Samuel Kahn. Lea and Febiger, Philadelphia. $7.50. 9¼ x 5⅞; 613; 1937.

This symposium by 24 eminent physicians summarizes and reviews critically the data on the rôle of trauma as an etiologic factor in the development and progression of disease. The purpose of the book is to furnish information of the sort needed in legal medicine and industrial hygiene. The more important disorders of all the organ systems are discussed and for each the task has been assigned to a well-known specialist. With but few exceptions, the subject has been treated in a thorough fashion, the general symptomology of a disease is summarized and then the evidence regarding the effects of trauma on it and the clinical characteristics of this effect, are presented clearly and concisely. Each chapter is supplemented by an adequate bibliography. Not only the physician but the student of vital statistics and of public health will find this a useful book to consult.

TAKE CARE OF YOURSELF. *A Practical Guide to Health and Beauty. Stressing the Proper Way to Use and the Prudent Way to Buy Home Remedies and Cosmetics.*

By Jerome W. Ephraim. Foreword by Logan Clendening. Simon and Schuster, New York. $2.00. 8 x 5½; xvi + 287; 1937.

As we flounder in the bewilderment of torrential advertising in the field of home remedies, cure-alls, and personal beautifiers, we welcome a book of this type. Whether it be insomnia, bunions, or "hang-over" that we are suffering from, *Take Care of Yourself* has some relief to offer. Mr. Ephraim is neither a physician nor a chemist, but he has spent much of his life in determining the value of many of our home remedies and cosmetics, and in advising the public as to the proper way to buy and use them. Although the author himself is a manufacturer and seller of drugs, cosmetics and toilet articles, he very cautiously refrains both from advertising his own brands, and from attacking the wares of his competitors. As Logan Clendening says in his forward, "Mr. Ephraim's statements . . . . stand exactly between the extremes of the advertisers and the debunkers of cosmetics and drugs."

The table of contents and topic index add to the value of the book as a home reference.

L'ANAPHYLAXIE *Expérimentale et Humaine. By Pasteur Vallery-Radot, G. Mauric and Mme Holtzer (ex-Hugo). Masson et Cie, Paris.* 36 francs. 9¾ x 6½; 130; 1937 (paper).

The first part, comprising about two-thirds of this book, is a report of experiments on the phenomena of allergy and desensitization. Most of the experiments were performed on rabbits, and the proteins used were those most commonly allergic to man via the respiratory tract, namely, squama from horses, cat hair, orchard grass, and wheat flour. The records include the period from injection of the sensitizing substance to the time of desensitization. The second part is a brief and rather sketchy treatment of anaphylactic shock in man, skin reactions to various proteins, and treatment. There is little critical discussion of the work of others on this subject, and the bibliography is confined to publications from the senior author's laboratory. There is no index.

THE ENDOCRINES IN OBSTETRICS AND GYNECOLOGY.

By Raphael Kurzrok. Williams & Wilkins Co., Baltimore. $7.50. 9 x 6; xvi + 488; 1937.

Although this book presents applications of recent discoveries of endocrine physiology to the problems of obstetrics and gynecology, in particular, it should also be of interest to biochemists, general practitioners, surgeons, and biologists. It combines research, by the author and others, on the problems of sex and reproduction with clinical observations made on a very large material at Columbia Medical Center and at the Bronx and Morrisania City Hospitals. Most of the material follows human physiology, illustrations from animal physiology being utilized only when similar observations have not been made on man. Besides a treatment of all the hormones and endocrines the author includes material on menstruation, ovulation, amenorrhea, and sterility. Bibliographies are incorporated in each chapter, and author and subject indices are provided.

THE EFFECT OF VOLUNTARY ACTIVITY ON THE KNEE-JERK. *Comparative Psychology Monographs, Volume 14, No. 4, Serial No. 70.*

By Sarah C. Dunlap. The Johns Hopkins Press, Baltimore. $1.25. 10 x 7; 62; 1937 (paper.)

The problem of the investigation reported upon this monograph was "to determine whether a leg response practiced to a knee-jerk stimulus or its tactual component can become involuntary, and whether the reaction time of such a response may approach with practice that of the knee-jerk; and further, what effect the practice may have on the knee-jerk time." Tentative conclusions to the problem are drawn on the basis of the data obtained from fourteen subjects in the experimental set-ups described. In the author's opinion it is obvious that the knee-jerk, in the normal individual, is no simple segmental "reflex", but an integrated response, of a complex nature.

LA PATHOGÉNIE DES OEDÈMES. *Confrontation des Théories a la Clinique.*

By Pierre Mauriac. Masson et Cie, Paris. 16 francs. 7¾ x 5⅜; 87; 1937 (paper).

The authors develop the thesis that transudative and exudative oedemas are produced by certain physico-chemical conditions in the tissues and humors of the body. The most important of these are disorders of the blood and lymph supply, mineral equilibrium, and metabolism of proteins and lipides, variations of pH and modifications of the capillary walls. These conditions, in turn, may be the consequence of cardio-vascular, renal and nervous disease, endocrine equilibrium or alimentary insufficiency. There appears to be no relation between pathogenic factors and the clinical type of oedema ultimately manifested. A bibliography has been supplied.

UNE FORME CÉRÉBRALE DE LA CHOLESTÉRINOSE GÉNÉRALISÉE (*Type Particulier de Lipose à Cholestérine*).

By Ludo van Bogaert, Hans J. Scherer and Émile Epstein. Masson et Cie, Paris. 45 francs. 10¾ x 6¾; 183; 1937.

A detailed and thorough account of this particular form of cerebral cholesterinosis, which is rather rare, is set forth here for the first time. In the first chapter the authors give the definition, history and classification of known forms of lipidosis. The second chapter gives a detailed clinical description of the form under consideration and discusses the possible importance of inherited constitutional factors. The third chapter is devoted to its histopathological characteristics and the fourth and last chapter is a biochemical and histochemical study of the deposits. This malady is apparently due to a metabolic maladjustment and not to an infection.

LES IMMUNITÉS LOCALES.

By A. Besredka. Masson et Cie, Paris. 35 francs. 9 x 5¾; 224; 1937 (paper).

The author's first book on this subject, published a little over a decade ago and now out of print, revolutionized the views on the biology of immunity. The present

volume, continuing the same thesis, presents newer findings, both experimental and clinical, on infections and the specific powers of antiviruses. The material is arranged according to organs, special sections being devoted to immunities of the skin, lungs, pleura, peritoneum, and intestines. One chapter treats plants in a similar way. Bibliographies conclude the separate chapters, and a detailed table of contents serves as index.

AN OUTLINE OF GENERAL PHYSIOLOGY.

By L. V. Heilbrunn. W. B. Saunders Company, Philadelphia and London. $5.00. 9 x 5¾; 603; 1937.

Dr. Heilbrunn prefaces this text with a quotation from Pavlov:

"... we are not far from a complete understanding of life as an association of organs ... the beginning, the basis of life is in the cell." With this idea in mind, he has devoted a very large portion of the book to considerations of cellular metabolism.

The descriptions and explanations are clearly written and easy to read. A great number of references are given in footnotes which are listed in an alphabetical author index in the back of the book. The subject index is also very thorough.

THE PATIENT AND THE WEATHER. *Volume IV, Part 2, Organic Disease. Hypo- and Hyperthyroidism, Diabetes, the Blood Dyscrasias, Tuberculosis.*

By William F. Petersen and Margaret E. Milliken. Edwards Bros., Ann Arbor, Mich. $11.00. 10¾ x 8¼; xxviii + 729; 1937.

Dr. Petersen's latest lengthy volume is in every respect similar to his previous ones which have been reviewed in this journal. Many new case records are presented of the type indicated in the sub-title. We cannot see how they add much evidence in support of his theory and while the descriptive material is straightforward and clear, his analyses in terms of weather is sketchy and confusing to say the least. Since he is doing so much work on the subject it is a pity that he does not take a large random sample of cases and present in tabular form the degree to which their physiological variations fit in with the variations in the weather.

LEÇONS DE PHYSIOLOGIE MÉDICO-CHIRURGICALE. (*Deuxième Série*).

By Léon Binet. Masson et Cie, Paris. 36 francs. 10 x 6½; 137; 1937 (paper).

The subject matter of this book is confined to a few points of immediate practical interest to the physician and especially the surgeon. Most of these are interestingly presented as experimental studies discussed in much more detail than is usually found in a text book. While it would not suffice for a course in physiology it should be a useful supplement to a standard, broad text book for students who intend to become surgeons.

L'HORMONE FOLLICULAIRE EN PHYSIOLOGIE NORMALE ET PATHOLOGIQUE. *Étude Expérimentale Clinique et Thérapeutique.*

By Henri Simonnet. Masson et Cie, Paris. 100 francs. 9¾ x 6¾; xii + 532; 1937 paper).

The author's aim is to demonstrate in what manner it is possible to conceive the rôle played by folliculin, in the normal state, in metabolism, and in disorders of which this hormone may probably be the cause. It is based on a voluminous literature (the bibliography runs to 2500 titles) of research and clinical observations, including some of his own work on the subject. Of interest to workers on various phases of sex and reproduction.

DER BLUTDRUCK DES MENSCHEN.

By Eskil Kylin. Theodor Steinkopff, Dresden and Leipzig. RM. 24 (paper); RM. 26 (cloth); 25 per cent reduction outside of Germany. 9½ x 6½; xv + 322; 1937.

This work is not intended to serve as a textbook or manual for the student or practising physician but rather to give a complete survey of the state of our knowl-

edge in the whole field of arterial tension and its regulation. Of great value is the literature list (pp. 262–316) which contains practically everything that has been written of real importance on the subject. The book is provided with illustrations and an index.

OXYGÉNOTHÉRAPIE ET CARBOTHÉRAPIE. *Bases Physiologiques. Applications Cliniques. Techniques.*

By L. Dautrebande. Masson et Cie, Paris. 35 francs. 9¾ x 6⅞; 300; 1937 (paper).

This book will be of interest to the physiologist, physician or surgeon rather than to the biologist. It treats the physiological bases of the requirement and transport of oxygen and carbon in the human body and their administration in the treatment of cardiac, circulatory, respiratory, and nervous diseases. The bibliography contains over 1200 titles. There is no index.

MATERNAL CARE. *The Principles of Antepartum, Intrapartum, and Postpartum Care for the Practitioner of Obstetrics. Approved by The American Committee on Maternal Welfare, Inc.*

Edited by F. L. Adair. University of Chicago Press, Chicago. 25 cents (paper); $1.00 (bound). 7⅝ x 5⅛; v + 93; 1937.

It would not be an exaggeration to state that every medical practitioner who takes care of maternity cases will find this booklet highly useful. Of special note are the detailed instructions for the delivery of women in their own homes. Information is given for forceps operations, version and extraction, and breech delivery. The material is very well organized, and the presentation exceptionally good.

LES PHÉNOMÈNES ELECTRODYNAMIQUES DANS LE SANG ET LE MOYEN DE LES DIRIGER.

By A.-L. Tchijevsky. Éditions Hippocrate, Paris. 10 francs. 10 x 6¼; 47; 1936 (paper).

This paper is a summary of the principles which have guided the author's work during the last 15 years in an effort to correlate the electrodynamic phenomena of the blood in states of health and disease. Although this work originated in Moscow it resembles in certain respects the teachings of Dr. George Crile in this country.

ANNALI DELL'ISTITUTO "CARLO FORLANINI". *Pubblicazione Mensile. Anno 1. Numero 1, 2, 3. 1937.*

Attilio Omodei-Zorini, Editor. Istituto Nazionale Fascista della Previdenza Sociale, Gennaio. Annual subscription: L. 50 (Italy and Colonies); L. 70 (outside of Italy); L. 30 (special price for physicians belonging to the I.N.F.P.S.). Single number: L. 5. 9⅜ x 6⅝; Numero 1, 82; Numero 2, 95; Numero 3, 116; 1937 (paper).

I MEDICINALI E IL METODO RINALDI PER LA CURA DELLE ARTRITI.

By D. Marotta, G. Lazzarini and A. Calò. Ministero dell' Interno Istituto di Sanita Pubblica, Viale Regina Margherita, 299, Roma. 10½ x 7½; 38; 1937 (paper).

BIOCHEMISTRY

ANNUAL REVIEW OF BIOCHEMISTRY. *Volume VI.*

Edited by James M. Luck. Annual Review of Biochemistry, Stanford University P. O., Calif. $5.00. 8¾ x 6; ix + 708; 1937.

It is always a pleasure to note in our columns this *Annual Review*. The topics in the volume (28 in number) have been well chosen and, as always in previous issues, have been presented by outstanding authorities. Of great value are the extensive bibliographies that accompany each section. It is the policy of the editors to include each year a few reviews on subjects of timely interest and in this volume two such are given—one on the "Application of microchemistry to biochemical analysis" by P. L. Kirk, of the University of California, and the other on the "Biochemistry of fish", by C. M. McCay, of

Cornell University. With this volume an important innovation has been started: namely, the inclusion of a subject as well as an author's index. This adds immeasurably to the usefulness of the book—particularly to those who do not belong in the ranks of the biochemist, but who, for one reason or another, have need to catch up on recent developments in this field.

MÉCANISME DES RÉACTIONS FERMENTAIRES. *Son Étude sur l'Amylase et l'Invertine.*

By L. Ambard and S. Trautmann. Masson et Cie, Paris. 35 francs. 9½ x 6½; 103; 1937 (paper).

The idea developed in this book is that in the action of a ferment upon a substrate the total reaction time is a sum of the durations of time required for three consecutive processes. The authors present their results of studies on the factors which account for the variation in the rapidity of these phases when amylase or invertin is used. There is no bibliography.

ANNUAL REVIEW OF BIOCHEMICAL AND ALLIED RESEARCH IN INDIA. *Volume 7, 1936.*

Society of Biological Chemists, India, Bangalore. Rs. 2 or 3s. (postage extra). 8⅜ x 5⅜; 165; 1936.

SEX

SEXUAL POWER.

By Chester T. Stone. D. Appleton-Century Co., New York. $1.50. 7⅜ x 5; vi + 172; 1937.

SEX LIFE IN MARRIAGE.

By Oliver M. Butterfield. Foreword by Sophia J. Kleegman. Emerson Books, New York. $2.00. 7⅝ x 5¼; xxi + 192; 1937.

SEX IN RELIGION. *An Historical Survey.*

By G. Simpson Marr. George Allen and Unwin, Ltd., London. 7s. 6d. 7¾ x 5; 279; 1936.

Sexual Power contains the customary chapters on impotence, psychic basis of sexual difficulties, physical basis of sexual difficulties, etc., forming an excellent rehash that does not appear to differ noticeably from its predecessors on the same subject.

It seems to us that one of the reasons why so many of the sex books are as inadequate as they are is because the authors were high-minded persons, or rather high-minded persons of the variety who believe that sexual intercourse should be a strictly altruistic enterprise on the part of both of the participants. In the second place, the invariable rule has been to devote a great deal of space to the elements of sexual calisthenics, and neglect the intermediate and higher branches entirely. Thirdly, it is assumed that the reader is a singularly unimaginative, not to say, stupid person. As an illustration of the latter point we quote from *Planning the Honeymoon*, a chapter in Mr. Butterfield's book, as follows: "The best bed is one that is not too hard, nor yet so soft that it wiggles and shakes all over when one moves a hand or foot. If one of a couple is accustomed to a very soft and the other a moderately hard bed, or if one needs many covers and the other few, it will be necessary to compromise a little to arrive at a satisfactory arrangement."

In the introductory chapter of *Sex in Religion* appears the following:

> "There is a feeling abroad today, a feeling which is steadily growing in intensity, that the Church has failed in her duty in so far as she has not dealt fairly with this relationship between sex and religion; and there is a growing desire amongst real friends of the Church that the various problems raised by sex at the present time, including the problems of marriage, divorce, and birth control, should be faced frankly and fearlessly by those in authority."

The sex factor in human affairs is traced from its manifestations in primitive religion to the viewpoints that the author regards as characteristic of modern times. While it is true, as the author so ably illustrates, that the sum total of happiness could be measurably increased by modifying the sex *mores*, it is a little difficult to believe that the increment would be as large as envisaged here. Perhaps too much consideration has been given to the lamentations of sex reformers, many of whom give every indication of believing that there are no maladjustments other than sexual.

"Under the freer and happier condi-

tions such as exist to-day Keats might have lived a very different life"—and might also have written bad poetry.

La Greffe Testiculaire du Singe à l'Homme. *Technique opératoire. Evolution Histologique. Manifestations Physiologiques. Extrait de "Technique Chirurgicale", No. 4, Juin, 1937.*

By Serge Voronoff. Doin et Cie, Paris. 9½ x 6¼; 20; 1937 (paper).

[English translation of above under title "Testicular Grafting from Ape to Man." Pp. 15.]

For any graft to be successful, the site of implantation should be as nearly identical to the original location of the tissue as possible. The author therefore decided some years ago that testicular tissue should if possible be placed within the scrotum of the receiver. Since there is considerable injury if an incision is made in the testicular pulp, the grafts were made to the external surface of the parietal layer of the tunica vaginalis. The technique is described in detail in this volume.

Many of those grafts have survived for as long as six years and living sperm have been seen in the tissue several years after transplantation. The author claims that "blood pressure invariably declines in cases of hypertension," that general tonus is raised, that the skin becomes firmer and more elastic, and that there are many other favorable changes both of the nervous system and of general metabolism which last for about six years when "grafting should then be repeated." Similar transplantations of simian parathyroid glands are likewise successful and a case is reported in which a young man suffering from tetanus was cured for a period of eight years.

From the general biological standpoint, the fact of long survival of the simian tissue and the mingling of its cells with those of the human species is more interesting and startling than the claimed physiological results. All the experiments in grafting of animal tissue show that autografts usually succeed and that homografts frequently succeed but that heterografts always fail to persist. In some cases grafting between very closely related species like the hare and the rabbit can be accomplished, the term homograft being applied to this type by Dartigues. Does the long survival of the tissues of higher apes in man then indicate closer relationship than is generally believed to exist?

Réunons Médico-Chirurgicales de Morphologie. *Morphologie Générale et Spéciale, Chirurie Réparatrice et Plastique, Endocrinologie—Dermatologie, Médicine Sportive et Hygiene Orthopédie—Physiothérapie, etc. Comptes Rendus des Séances, Année 1, No. 4, Décembre, 1936.*

Dr. Claoué, 19, rue Scheffer, Paris. 50 francs (in France); 100 francs (outside of France). 9½ x 6; 89–179; 1936 (paper).

Contains interesting paper (pp. 92–152) on sexual mutilations by Henri Allaix.

BIOMETRY

Five Place Tables. *Logarithms of Integers, Logarithms and Natural Values of Trigonometric Functions in the Decimal System for Each Grade from 0 to 100 Grades with Interpolation Tables.*

By P. Wijdenes. P. Noordhoff, Groningen. Fl. 2.50. 9¼ x 6; 168; 1937.

To the astronomers of the valley of the Euphrates we owe the usual method of measuring angles and their subtending arcs, a method based on the sexagesimal division of the sextant. A somewhat improved method which has never enjoyed the popularity which it merits, based on the decimal division of the quadrant, was proposed by the same Congress that formulated the metric system.

The present work is a series of tables of natural and logarithmic trigonometric functions based on this latter system with one hundred grades to the quadrant, subdivided into desigrades, centigrades, and milligrades. There are also supplementary tables for the interconversion of degrees, grades, and radians.

While the present reviewer is glad to

concede the superiority for all purposes of the grade over the degree, he cannot feel optimistic about the chances of acceptance of these tables. The average man is a creature of habit, and is likely to continue measuring his angles, if any, with degrees because his ancestors have always done so.

PRINCIPLES OF MEDICAL STATISTICS.

By A. Bradford Hill. The Lancet Limited, London. 6s. 8½ x 5½; vii + 171; 1937.

The substance of this book appeared in the form of a series of articles recently published in *The Lancet*. They were written for the express purpose of giving physicians a clear idea of the principles of statistical analysis. The enthusiasm with which the articles were received is sufficient testimony of their merit. Without doubt the author has achieved his purpose. He has limited himself to a description of the simplest analytical procedures but these should be sufficient for the general run of clinical investigations. The exposition is clear and involves nothing in the matter of mathematics which could not be understood by the reader of average culture. The sections which in particular seem praiseworthy are those in which the author discusses the selection of samples and certain common fallacies of statistical application. As an introduction to statistics this little book may be highly recommended.

THE DESIGN OF EXPERIMENTS. *Second Edition.*

By R. A. Fisher. Oliver and Boyd, Edinburgh. 12s. 6d. net. 8¾ x 5½; ix + 260; 1937.

Professor Fisher's well-known book *Statistical Methods for Research Workers* deals not only with statistical methods but with the problem of planning experiments so as to furnish as much information as possible on the questions which the experimenter wishes to answer. This book deals with the latter question in more detail. It is often stated that in an experiment all of the independent variables but one should be held constant. As Fisher shows, and others have been aware before, this is not only often impossible but undesirable. By proper planning of an experiment just as reliable information may be obtained regarding the effect of several variables on the dependent variable as could be obtained for only one independent variable.

MATHEMATISCHE METHODEN DER BIOLOGIE *insbesondere der Vererbungslehre und der Rassenforschung.*

By Friedrich Ringleb. B. G. Teubner, Leipzig and Berlin. RM. 8.80 (In Germany); RM. 6.60 (Outside of Germany). 8½ x 6; vii + 181; 1937.

This excellent textbook of biometry gives in comparatively brief space an exposition of graphic methods, centering constants, variation, probability, mathematical foundations of genetics, Bernoullian, Gaussian, Poisson, and Lexis distributions, and correlation.

PSYCHOLOGY AND BEHAVIOR

THE QUESTIONING MIND. *A Survey of Philosophical Tendencies.*

By R. C. Lodge. E. P. Dutton and Co., New York. $2.75. 8½ x 5½; vii + 312; 1937.

This book is an unusually simple and clearly written introduction to some of the traditional topics of philosophy. It is perhaps too simple to be used as a textbook, but it is admirably suitable for the delectation of a reader who is unprepared or disinclined to struggle with a more profound treatment of philosophical problems.

Beginning with the notion that philosophical contemplations can emerge from reflection upon any kind of event or activity, scientific or mundane, the author proceeds to the consideration of epistemology, ethics, the mind, self, and education. These topics are approached from the positions of realism, idealism, and pragmatism. On the whole these three points of view are treated fairly, although occasionally the author expresses a bias. For example, the realistic approach to

ethics is described as an attempt to construct a system of human conduct, with the aid of mathematical logic, which will be a purely formal and abstract science of social physics, a mathematics of ethics. The result, according to the author, presents society as an hedonistic dog-eat-dog affair, and so abstract as to be unconnected with the actual life of human beings in social relationships.

Now it may be true that the logical analysis of ethical concepts is an ideal of certain realists, though assuredly not of all, but even for the few this desire has not been realized. Moreover, a scientific approach to ethics may well turn out to be the best way in which to study problems of considerable importance to our understanding of man.

In spite of these occasional unsatisfactory interpretations of standard philosophical positions, this book serves its purpose admirably well.

FEEDING BEHAVIOR OF INFANTS. *A Pediatric Approach to the Mental Hygiene of Early Life.*

By Arnold Gesell and Frances L. Ilg. J. B. Lippincott Co., Philadelphia. $4.50. 10¼ x 6¾; ix + 201; 1937.

The data presented in this volume supplement those of the previous publications by the senior author and his associates. Richly illustrated, this book contains a detailed account of the development in the feeding behavior of some 10 children in the first two years of life. Following a brief outline of the method of study, the essential feature of which is to make movie records of the infant in its own home environment, the authors describe the motor mechanisms involved in suckling, mastication, and swallowing. There follows a summary of the observations on feeding behavior with breast, bottle, and cup and spoon in the successive periods of the first year of life, and on the patterns of infantile reactions to the presentation of food when this is desired and when it is not. The third part of the book is dedicated to problems of adjustment to feeding and sleeping schedules and of training to feeding and hygienic habits. The broad experience of Gesell is here manifested by the sane outlook with which these problems are discussed. At all times the authors stress the importance of the individual and note that training schedules should be to some extent modified by the individual behavior characteristics which in turn are intimately related to the physiologic development of the infant.

This as well as previous publications constitutes a fundamental contribution towards accumulating objective information relative to infant behavior. It deserves to be read not only by pediatricians, psychologists, and human biologists but should be made available also to the layman since it is written in a clear and straight-forward manner. In an appendix are presented the detailed records of four children. There is a comprehensive bibliography and an index.

IN THE REALM OF MIND. *Nine Chapters on the Applications and Implications of Psychology.*

By Charles S. Myers. The University Press, Cambridge; Macmillan Company, New York. $2.50. 7¼ x 4¾; 251; 1937.

The nine chapters of this book are modifications of as many public lectures given by the author before numerous British Scientific Societies over a period of 7 years extending from 1929 to 1935. The nature of the book deprives it of any unity save that derived from the fact that all the discourses are organized around the modern trend in applied psychology. Dr. Myers discusses such topics as the human factor in accidents; the use of psychology in the choice of a career; a psychological regard of medical education; and psychological concepts in other sciences with an understanding and a simplicity that can come only with many years of keen observation and intensive study.

For the student or professor of general psychology, for the vocational advisor, and for the personnel director of industrial establishments alike, this volume will be both stimulating and enlightening. It is indeed unfortunate that a book of this

caliber should be devoid of a more complete table of contents and an index.

THE PARENT-CHILD RELATION: *the Psychological Background and Other Papers. Individual Psychology Medical Pamphlets No. 17. By H. G. Baynes, S. H. Lubner, A. C. Court, M. Marcus and F. G. Crookshank. C. W. Daniel Co., London.* 2s. 6d. 8¾ x 5½; 71; 1937 (paper).

This journal is the organ of the *Medical Society of Individual Psychology* in London. The late Alfred Adler was its honorary president and its purpose is to make the facts of individual psychology readily accessible to physicians in general practice.

The leading article is a discussion of the relationship of parent and child from the psychoanalytic point of view. In discussing this problem, the author spends a considerable part of time upon the incest problem of Hamlet, the life of D. H. Lawrence, and a novel by Franz Werfel. Supplementing the article is a discussion of childish dreams and their unbelievably deep significance. In addition, there is a series of testimonials by English physicians praising the benefits of individual psychology in their practice.

To the reader interested in individual psychology, the psychoanalytic technique, and the literary approach to science, the journal will be of interest.

FACTORS IN RAT LEARNING. *An Analysis of the Intercorrelations Between 34 Variables. Comparative Psychology Monographs, Volume 14, Number 3.*

By Charles L. Vaughn. The Johns Hopkins Press, Baltimore. 75 cents. 10 x 5¾; 41; 1937 (paper).

Ten different set-ups were used in the experiments, including three types of mazes. Four measurements were taken to represent the rat's performance in each situation: the number of entrances into culs-de-sac (errors) on a given number of trials; number of trials to meet a criterion; time spent in correct pathway for a given number of trials; and the time per error for those trials. Measurements of different aspects of the rat's behavior in the various situations, combined with age, weight losses, etc., make up the set of 34 variables. The factor analysis technique was used in working the data.

THE COOPERATIVE SOLVING OF PROBLEMS BY YOUNG CHIMPANZEES. *Comparative Psychology Monographs, Vol. 14, No. 2, Serial No. 68.*

By Meredith P. Crawford. The Johns Hopkins Press, Baltimore. $1.50. 10 x 6⅛; 88; 1937 (paper).

The aim of this investigation was "to discuss what natural modes of attack young chimpanzees might employ when presented with certain problems for cooperative solution, and to train them in the solution of such problems." The technique was to train each chimpanzee individually to perform certain manipulations with boxes, etc., and then alter the set-up to such an extent that it would now require the cooperation of two chimpanzees to carry out the same operations.

HYPNOTIC POWER: *Its Cultivation, Use, and Application to Psychotherapy.*

By Colin Bennett. E. P. Dutton and Co., New York. $1.50. 7½ x 5¼; 158; 1937.

This work is largely a description of hypnotic states, with advice to the inexperienced hypnotist as to their production, and their therapeutic use. It is not a treatise on hypnosis. It consists of thirty-one loosely connected chapters and no index.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

STUDIES IN HAND-READING.

By Charlotte Wolff. Preface by Aldous Huxley. Alfred A. Knopf, New York. $3.00. 8¾ x 5¾; xvi + 154 + 62 plates; 1937.

Dr. Wolff is a palmist, and also a physician, psychologist, social worker, and, incidentally, an admirer of Freud and Jung. All these accomplishments are supposed to make the lady a "scientific" palmist.

In this book she analyzes from their hands the characters of sixty people from servant girls to church dignitaries. Among the celebreties whose palm readings are included are T. S. Eliot, Bernard Shaw, and Anna May Wong. For every subject an imprint of the hand has been photographed as well as a small sketch illustrating only the most revealing characteristics.

In the manner of the old time practitioners of her art and mystery Dr. Wolff examines first the shape and proportion of the whole hand and then the character of the various lines on the palm. But instead of reading in them fortune, adventure, and maybe a future brunette lover, she sees such high toned things as "underdeveloped ovaries" (shown by lack of moons on the nails) and "sublimated erotic desires" (revealed by the ring of Venus).

In the preface Aldous Huxley enthusiastically says: "Nobody who has had a sitting with Dr. Wolff, for example, can doubt her ability to make a diagnosis of physical conditions and tendencies that is often astonishingly detailed and accurate; can doubt her power to specify past events and date them correctly to within a few months; can doubt her knack of describing character, and the secret springs of action with a penetrating and often disturbing insight." [Reginald, the Office Boy, says that right off he can name two people who would be able to pump up that much skepticism, sitting or no sitting.]

However, Huxley carefully avoids saying whether Dr. Wolff's interesting character studies are actually derived from a study of the hand alone, or with the aid of a lively imagination and a keen feminine intuition, or just possibly the merest smidgen of previous knowledge of the character to be unveiled. But such a low suspicion is unworthy; "scientific" palmists of course always take particular pains to keep from knowing anything about customers until they hold their hands.

Literate ♀ gypsies will find this a useful book.

TYPING ACADEMIC PAPERS. *A Manual and a Model for the Author and Typist.* *By W. K. Cunningham, Jr., and Ben M. Patrick. Edwards Brothers, Ann Arbor, Mich.* $1.30. 8¼ x 5¼; xii + 118 + 6; 1937 (paper).

A useful guide, especially for the beginner. Like all such books which do not pretend to be all-inclusive there are omissions—yet this reviewer gleaned bits of information from its 100 or so pages that he had been unable to find in more comprehensive treatises.